

PROCEEDINGS

THE INSTITUTION OF CIVIL ENGINEERS

PART I
MARCH 1953

ORDINARY MEETING

18 November, 1952

HENRY FRANCIS CRONIN, C.B.E., M.C., B.Sc., President, in the Chair

The President said that he very much regretted to have to inform the Meeting of the recent death of Mr Sydney Bryan Donkin, who had been President of the Institution during the year 1937-38. A Resolution of Condolence had been passed by the Council and conveyed to the members of his family.

The Council reported that they had recently transferred to the class of

Members

CUTHBERT LLEWELLYN CHAMPION, B.Sc. (Eng.) (Lond.).	JOHN COUCH ADAMS ROSEVEARE, <i>Jun.</i> , D.S.O., B.Sc. (Eng.) (Lond.).
FRANK HOWARD CLINCH, B.Sc. (Eng.) (Lond.).	HAROLD KYLE SCOTT, M.B.E., B.Sc. (Belfast).
DAVID GORDON DRUMMOND, B.Sc. (Eng.) (Lond.).	

and had admitted as

Graduates

ALAN MICHAEL ADYE, B.A. (Cantab.), Stud.I.C.E.	PETER WILLMOT BEARD, B.Sc. (Eng.) (Lond.), Stud.I.C.E.
DAVID ALLEN, B.Sc. (Bristol), Stud.I.C.E.	OLIVER MAURICE BEVAN, B.Sc. (Eng.) (Lond.).
ERIC ANDERSON, B.Sc. (Eng.) (Lond.), Stud.I.C.E.	WALTER STEWART BELL BOND, B.Sc. (Belfast).
JOHN STANLEY BARNETT, B.E. (Tas- mania).	RICHARD STEVEN BORLAND, Stud.I.C.E.

- BRIAN CHARLES BRAZIER, B.A. (*Cantab.*).
 JOHN JOSEPH BREWER, Stud.I.C.E.
 WILSON BROWN, B.Sc. (*Glas.*).
 ALAN ERNEST BULLOCK, B.Eng. (*Sheffield*), Stud.I.C.E.
 IAN NEIL BUTTERFIELD, B.Sc. (*Natal*).
 ANTHONY CHARLES EDWIN CALDICOTT, Stud.I.C.E.
 ROY FRANCIS CARTER, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 THEODORE KING CHAPLIN, M.A. (*Cantab.*), Stud.I.C.E.
 RONALD EDGAR CLEAVER, B.Sc. (*Birmingham*), Stud.I.C.E.
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 GEOFFREY COWEY, B.Sc. (*Durham*), Stud.I.C.E.
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 JOHN DAVIE, *Jun.*, B.Sc. (*Glas.*), Stud.I.C.E.
 WILLIAM DAVID DAVIES, B.Sc. (*Wales*).
 RONALD STEPHEN DAYA, Stud.I.C.E.
 MICHAEL GEORGE DERRINGTON, B.Sc. (*St Andrews*).
 GEORGE SMITH DODDS, Stud.I.C.E.
 JOHN HOWARD DODWELL, Stud.I.C.E.
 ELIAS MARCUS DUKE-COHAN, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 MICHAEL JOHN DULIGALL, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
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 ALLAN ROCHESTER GENT, Ph.D., B.Sc. (*Durham*).
 OSWALD GIBB, M.Eng. (*Liverpool*).
 ALEXANDER MCINTOSH GILL, B.Sc. (*Aberdeen*), Stud.I.C.E.
 JOHN WILLAN GLOVER, Stud.I.C.E.
 JAMES TERENCE GREGG, B.Sc. (*Belfast*), Stud.I.C.E.
 PETER JAMES GREGORY, Stud.I.C.E.
 TIMOTHY RUSSELL GURNEY, B.A. (*Cantab.*).
 BERNARD HALL, B.Sc. (*Nottingham*).
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 PETER HEYWOOD, B.Eng. (*Liverpool*), Stud.I.C.E.
 WILLIAM HUTTON HINSELWOOD, Stud.I.C.E.
 BERNARD HOWARTH, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 IVOR HOWDEN, B.Sc. (Eng.) (*Lond.*).
 STANLEY MCKENZIE HOY, Stud.I.C.E.
 PHILIP AMBROSE HUTCHINGS, B.Sc. (Eng.) (*Lond.*).
 IAN DAVID HYND, B.Sc. (*St Andrews*).
 JOHN DEREK JACKSON, B.Sc. (*Durham*).
 JOHN MICHAEL JACKSON, B.Sc. (*Leeds*), Stud.I.C.E.
 RONALD FREDERICK JAMES, Stud.I.C.E.
 DAVID ERNEST JENKINS, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 WILLIAM SINGLETON KEABLE, B.Sc.Tech. (*Manchester*), Stud.I.C.E.
 DAVID HENRY EDWARD KIRKE, Stud.I.C.E.
 MICHAEL WILLIAM KNILL, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 NORMAN SAMUEL LINDSAY, Stud.I.C.E.
 JOHN DAVID DANIEL LLOYD, Stud.I.C.E.
 MAURICE JAMES LOWTHER, B.Sc. (*Belfast*), Stud.I.C.E.
 IAN GILROY MACKENZIE, Stud.I.C.E.
 IAN BRINE MACKINTOSH, M.A. (*Cantab.*), Stud.I.C.E.
 GEORGE MELVILLE McNEIL, B.Sc. (*Glasgow*), Stud.I.C.E.
 JOHN KEITH MACQUEEN, B.A. (*Cantab.*), Stud.I.C.E.
 PETER McWATT, B.Sc. (*Glas.*), Stud.I.C.E.
 ABDUL KADER MAMOOJEE, B.Sc. (*Bristol*).
 ROBERT BREWSTER MARKHAM, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 JOHN STUART MARTIN, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 HUBERT TREVOR DALRYMPLE MARWOOD, B.A. (*Cantab.*).
 FRANK HILLSON MEAD, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 HAROLD MEAKIN, B.Sc.Tech. (*Manchester*), Stud.I.C.E.
 JOHN EDWARD MITCHELL, B.A. (*Cantab.*).
 JOHN SLOAN MOORE, B.Sc. (*Belfast*), Stud.I.C.E.
 LEONID CECIL MORGAN, B.Sc. (*Wales*), Stud.I.C.E.
 COLIN KEITH MOULSON, B.Sc. (Eng.) (*Lond.*).
 ALAN GEORGE MUMBY, B.A. (*Cantab.*).
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 ALAN WILLIAM PAYTON, B.Sc. (Eng.) (*Lond.*).
 DAVID WILLIAM PILLAR, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 BRIAN JOHN PITT, B.E. (*Tasmania*).
 ROGER KELSALL POSTLETHWAITE, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 GEOFFREY HEDDON POTTER, Stud.I.C.E.

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 GLYN TREHARNE REES, Stud.I.C.E.
 JOHN MORTIMER REID, B.Sc. (*Aberdeen*).
 GRAHAM JAMES RITCHIE, B.Sc. (Eng.) (*Lond.*).
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 JOHN RICHARD ROSEWARNE, B.Sc. (*Wales*), Stud.I.C.E.
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 JOHN RYELL, Stud.I.C.E.
 JOSEPH STANLEY SALT, Stud.I.C.E.
 GORDON ALEXANDER GEORGE SAPSTEAD, Stud.I.C.E.
 JOHN WALKER SAUNDERSON, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 COLIN NICOL SCHOFIELD, B.Sc. (*Durham*).
 WILLIAM JOHN SCOTT, B.Sc.Tech. (*Manchester*), Stud.I.C.E.
 JOCK DOUGLAS SEMPILL, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 JOSEPH SYLVAN SHIELS, B.Sc. (*Belfast*), Stud.I.C.E.
 JOHN BRUCE SIMCOX, B.Sc.Tech. (*Manchester*), Stud.I.C.E.
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 RALPH ERIC SMITH, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 RONALD BERTRAM SNACK, Stud.I.C.E.
 JOHN KENNETH SODEN, B.Sc. (*Belfast*), Stud.I.C.E.
 KEITH CHARLES STANLEY, Stud.I.C.E.
 BERNARD STONES, B.Sc. (*Leeds*), Stud.I.C.E.
- WILLIAM EVARTS STREETEN, B.A. (*Can- tab.*), Stud.I.C.E.
 GRAHAM PHILIP STURTON, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 FREDERICK HERMAN SUCH, Stud.I.C.E.
 DAVID WILLIAM TAPPING, B.Sc. (Eng.) (*Lond.*).
 DAVID HALL TODD, Stud.I.C.E.
 MICHAEL FERNIHOUGH TONG, B.Sc. (*Manchester*), Stud.I.C.E.
 MICHAEL INGHAM TOWNSEND, B.Sc. (*Manchester*).
 LOUIS JAMES VITALI, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 WILLIAM STANLEY ADIEL WADSWORTH, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 HUBERT EATON WALKER, B.Eng. (*Liver- pool*), Stud.I.C.E.
 JAMES HERBERT WARDROPER, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 PETER HERBERT WARE.
 CHRISTOPHER HUGH WATSON, Stud.I.C.E.
 JOSEPH PHILIP WESTWELL, B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 GEORGE WILLIAM LEONARD WHEELER B.Sc. (Eng.) (*Lond.*), Stud.I.C.E.
 GEORGE THOMAS EUNSON WILSON, B.Sc. (*Edin.*), Stud.I.C.E.
 ALAN ALEXANDER WOODHEAD, B.A. (*Can- tab.*).
 CHARLES CHRISTOPHER WRIGHT, B.A. (*Can- tab.*).
 STUART CHALMERS WYLIE, B.Sc. (*Natal*), Stud.I.C.E.

and had admitted as

Students

- REGINALD CHARLES AMOS.
 FRANK ALBERT COWPER AMOTT.
 THOMAS ALFRED ANDERSON.
 MICHAEL ANNELLS.
 JAMES DAVID ANTHONY ARUNDEL.
 BRYAN HENRY BARTRAM.
 JOHN GORDON BOND.
 DAVID BRASH.
 DONALD GEOFFREY BRUFF.
 JOHN CHARLES BRUNTON.
 PETER DONALD CAVANAGH.
 CHEAH THEAN SUN.
 CHEN ANDREW CHIA-TUNG.
 CHIN KEE KHEONG.
 CHUA NGOH CHUAN.
 JACK CONNELL.
 MICHAEL HENRY DENT.
 MALCOLM ROVERY DOUGLAS.
 CHRISTOPHER THOMAS DUFFY.
 PAUL EDMOND DUNLEAVEY.
- CHRISTOPHER BRIAN EDWARDS.
 PETER REX EDWARDS.
 PETER JOHN WILLIAM GAINSFORD.
 PETER DENIS HALL.
 JAMES HANNAH.
 LIONEL DUDLEY HARKER.
 COLIN WEDGWOOD HOOD.
 MICHAEL JOHN BARTON HORNE.
 ZYGMUNT IWANSKI.
 MARTIN DUDLEY JAPES.
 ROYSTON VIVIAN JENKINS.
 CHARLES NAPIER CARRUTHERS JOHN-
 STONE.
 CHARLES LLOYD JONES.
 KATHIRAVELU KANAGARATNAM.
 ALAN HARRY KLEIN.
 JAMES FREDERICK KNIGHT.
 BERNARD ARTHUR LEACH.
 LEE TECK CHIEW.
 PETER CLAMPITT LOVEYS.

ARTHUR EDWARD LOW.
ALAN HUGH MCCARTNEY.
MAHAMED ALI MAGAN.
DENYS LE RUEZ MARETT.
MICHAEL JOHN MILLER.
GERALD JOHN MORGAN.
ANTHONY MARK MUNSCHE.
ERROL DAVID NORDIN.
DAVID CHARLES NOWELL.
JOHN RAYMOND NUTTER.
MICHAEL PATRICK O'DWYER.
DENNIS VINCENT O'GRADY.
JOSEPH MARTIN PARKER.
JOHN FRANCIS HENRY PITTMAN.
PIETER JACOBUS CHRISTOFFEL STEFANUS
POTGIETER.
DENIS ROY PURDIE.
RAJALINGHAM RADHAKRISHNA.

COLIN JAMES RAINSFORD-MOORE.
GORDON JOHN REED.
PATRICK JAMES REID.
ALEX SYDNEY RHODES.
ANTHONY JOHN SIBLEY.
JAMES ROXBY SIMPSON.
HARDYAL SINGH.
ALAN SMITH.
JAMES DIXON SMITH.
RONALD SMITH.
STANLEY JOHN STAINSBY.
JOHN GILBERT SYMINGTON.
JOHN WILLIAM TARLING.
ERIC THOMPSON.
ANTHONY EDWARD WATTS.
HERBERT ROY WHATLEY.
LAWRENCE JOHN WOODHEAD.
GORDON EDWARD YOUNGHUSBAND.

The following Paper was presented for discussion and, on the motion of the President, the thanks of the Institution were accorded to the Authors.

Paper No. 5882

“Creep of High-Tensile Steel Wire”*

by

**Noel William Bailey Clarke, M.Eng., M.I.C.E., and
Francis Walley, M.Sc., A.M.I.C.E.**

SYNOPSIS

The Paper describes experimental work carried out in order to determine quantitatively the losses which occur in stress in a cold-worked high-tensile steel wire when stretched at a constant or near-constant length. The apparatus used is described and the stress loss is shown to be a function of the ratio of the applied stress to the proof or ultimate stress. Published data on the creep of this type of wire are briefly examined. The application of this work to prestressed concrete is discussed.

INTRODUCTION

WITH the advent of prestressed concrete, the use of very-high-tensile steel in conjunction with concrete to form a structural member became a possibility. In addition, it was quickly shown that the same factors of safety as used with normal mild steel need not be applied to the steel used in prestressed concrete, since the stress in the steel when the beam is carrying its design load is usually less than the initial stress put into the steel. The material is thus tested during construction.

The upper limiting factor in the choice of a suitable steel stress is not therefore governed by the increase of stress due to the applied design load. It is, of course, controlled to a large extent by the fact that it is desirable to have a factor of safety in the member of between 2 and 3, and calculation¹ shows that in the case of simple prestressed rectangular beams, where the design stress in the concrete is one-quarter of its ultimate prism strength, the initial stress in the steel should not exceed about three-quarters of its ultimate strength if a factor of safety of $2\frac{1}{2}$ is desired.

The use of such high stresses, however, introduces another complication—the creep of the steel. The phenomenon of creep is well known in metals at high temperatures, particularly the softer metals. The phenomenon at normal temperatures is not so well known, but it may be important where high-tensile steel is used at stresses high in relation to its

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¹ The references are given on p. 135.

ultimate strength, as in prestressed concrete. Since it was considered that this information was largely lacking, a programme of tests was put in hand at the Field Test Unit* of the Ministry of Works, and the work is described in the following pages.

PREVIOUS EXPERIMENTAL WORK

The first intimation that the creep of steel might be an important factor in prestressed concrete was given by Professor Magnel,² when he described tests carried out on 5- and 7-mm.-diameter high-tensile steel wire in his laboratory in order to determine the relaxation of the wire at constant length and also its increase in length at constant stress. He also investigated the effects of over-stretching the wire.

His results showed three things :—

- (a) That the loss of stress at constant length and the increase in length at constant stress could be large.
- (b) That it could be reduced to a small amount by increasing the load in the wire by 10 per cent for a short duration.
- (c) That it was possible to obtain a steel which had very small creep loss at the stresses used in prestressed work.

These tests were naturally carried out on Belgian steel, and it was considered that the same conclusions could not necessarily be drawn for British steel, particularly in the light of work by other investigators, which did not give the same quantitative results.

Attention was also drawn to this subject by Swiss engineers³ in 1946, but very few test results are given and reliance is placed on a formula similar to the following :—

$$e_k = \frac{1}{10} \left(\frac{\sigma}{0.45\sigma_s} - 1 \right)^2$$

where σ_s denotes the 0.2-per-cent proof stress of the steel,

σ „ the initial stress in the steel, and

e_k „ the increase in strain at constant load, expressed as a percentage.

The recommendation in the Swiss report is that this formula should be used if the creep properties of the wire are not known. Its accuracy in the light of the Authors' results is examined in a later section.

A further contribution to this subject was made by Dawance.⁴ In his work he used short lengths of wire about 1 metre long held at constant length, but with different initial stresses, and he determined the loss of stress by measuring its frequency. Some of the results given are interesting in that they cover a very long period of time (2 years or more). Again

* Now the Building Operations Research Unit of the Building Research Station, Department of Scientific and Industrial Research.

they show that the creep of steel can be large. The results are examined in more detail later.

One further contribution may be noted, that of De Strycker.⁵

As a result of the perusal of these works it was decided that the steel itself may have played the most important part in creating the differences which could be observed in the results obtained by the different investigators. Thus it was imperative that some knowledge should be obtained of the possible creep losses of British steel if prestressed concrete was to become widespread and accepted as a method of construction in Great Britain.

CHOICE OF THE METHOD OF TESTING

As has been intimated, creep experiments can readily be carried out either at constant length or at constant stress. In addition, short- or long-length specimens may be used. It was eventually decided that creep experiments under constant length, that is, measuring the relaxation in stress, should be carried out with long lengths of wire. It was felt that this approximated more nearly to conditions in actual beams. Although concrete beams do shorten because of creep and shrinkage in the concrete, the movement is relatively small and the wires are held at approximately constant length. Long lengths of wire are more usual than short lengths in prestressed work, so arrangements were made to use approximately 40-foot lengths of wire. It was also desired that the tests should run for at least 1,000 hours. Constant-load tests have been used by the majority of workers in this field, but this type of test gives relatively high creep.

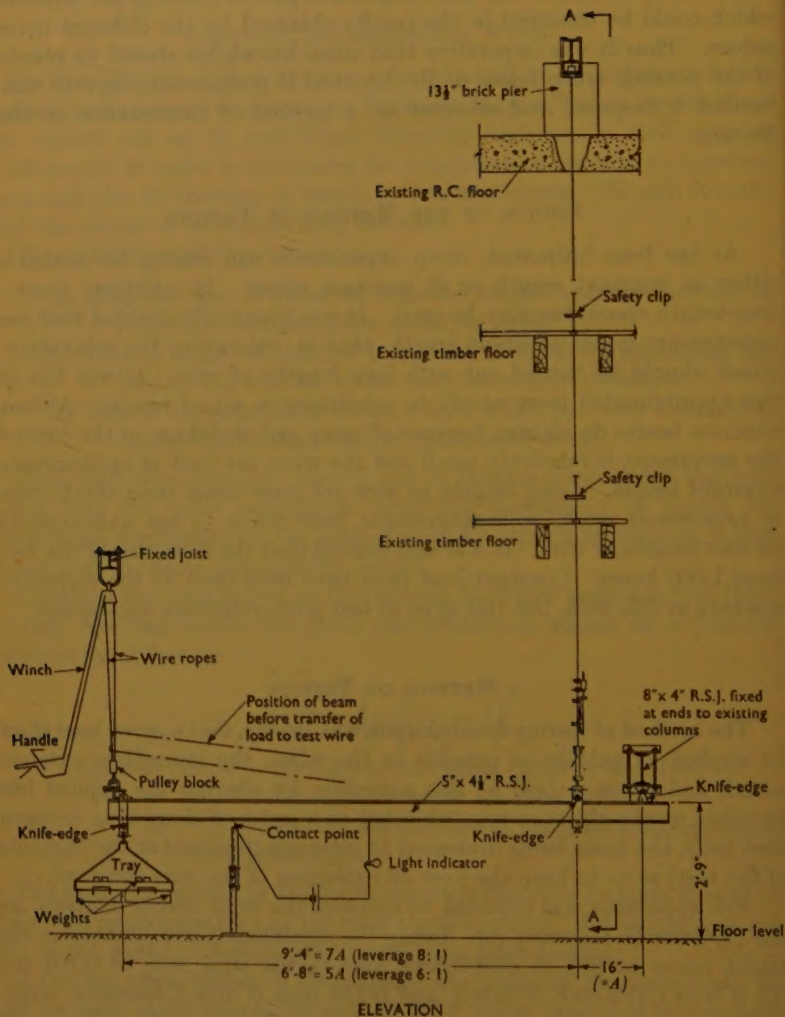
METHOD OF TESTING

The method of testing decided upon was, briefly, that a given load should be applied as quickly as possible to the wires, the immediate extension noted, and the wire held at this extension by altering the applied load. In other words, the wire was subjected to a series of short-time constant-load tests, the loads being decreased at intervals (frequent at the beginning of the test) so as to keep the over all extension of the wire constant.

The apparatus was required to measure the total effects of creep, and the variation in the creep rate during the significant period (whatever that might prove to be), in cold-drawn high-tensile steel wire of 0.104 inch (12 S.W.G.), 0.2 inch (5 mm.), and 0.276 inch (7 mm.) diameter, over a wide range of initial loadings and on a length of about 40 feet, first on the stress in the wire under conditions of constant length, and secondly, on the length of the wire under constant load.

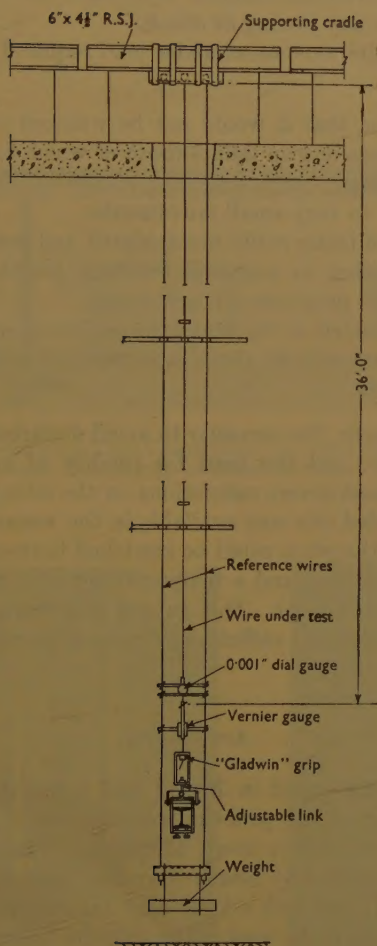
The fundamental requirements of the loading were therefore that it should be weighed positively to within ± 1 per cent and should be readily variable fractionally, constant between observations, and conveniently

Fig. 1 (a)



TEST RIG FOR CREEP TEST ON HIGH-TENSILE WIRE

Fig. 1 (b)



SECTION AA

TEST RIG FOR CREEP TEST ON HIGH-TENSILE WIRE

sustained over long periods ; also that the initial loading of the wire should be variable at will on different specimens over a wide range (actually 446 lb. to 2,163 lb. per wire on the 0.104-inch-diameter wire, 2,198 lb. to 5,960 lb. per wire on the 0.2-inch-diameter wire, and from 4,020 lb. to 10,000 lb. on the 0.276-inch-diameter wire), and that it should be applied quickly but without any jarring or shock.

The control of wire movement under load required that the apparatus should be :—

- (a) Robust, so that it would not be wrecked in the event of wire breakage or grip failure (both of which have occurred during the tests).
- (b) Sensitive to very small movements.
- (c) Capable of being easily manipulated and read.
- (d) Usable either to maintain constant length in the wire or to indicate progressive lengthening.
- (e) So constructed as to eliminate or detect errors owing to temperature, slip in grips, movement of anchorages, vibration, or shock.

The long test-length, the necessity to avoid disturbance during a period of weeks or months, and the need for rigidity of anchorages during a similar period, imposed severe restrictions on the siting of the apparatus.

A suitably secluded site was available in the water tower at the Field Test Unit in which the wires could be stretched between the heavy beams supporting the water tank and a temporary girder attached to the tower stanchions near ground level. This gave a test length of 36 feet for the wires, which was considered sufficient for present purposes.

APPARATUS

The apparatus illustrated in *Figs 1* and *2* was devised to meet the special requirements of the problem and the site. The loading device consists essentially of a heavy lever, designed to transmit load to one test wire and positively loaded by means of known weights carried on a knife-edge at the free end, and with a knife-edge fulcrum at the fixed end, that is, the end remote from the load. The fulcrum bears upwards against a beam firmly fixed near ground level (*Fig. 3*). Near the fulcrum a Gladwin wire-grip is carried in an adjustable stirrup which bears on the underside of the lever through another knife-edge. The top (fixed) end of the test wire is held in another Gladwin grip carried by one of the main beams at the top of the tower.

The leverage of the system is constant through the working arc of the lever, and so the load imposed on the wire by a given weight on the free end of the lever is also constant through the working movement. Four

Fig. 2



6-TO-1 LOADING LEVER WITH TEST COMMENCING ON 0.104-INCH-DIAMETER WIRE,
SHOWING ELECTRICAL CONTACT DEVICE IN USE

Fig. 3

Fig. 4

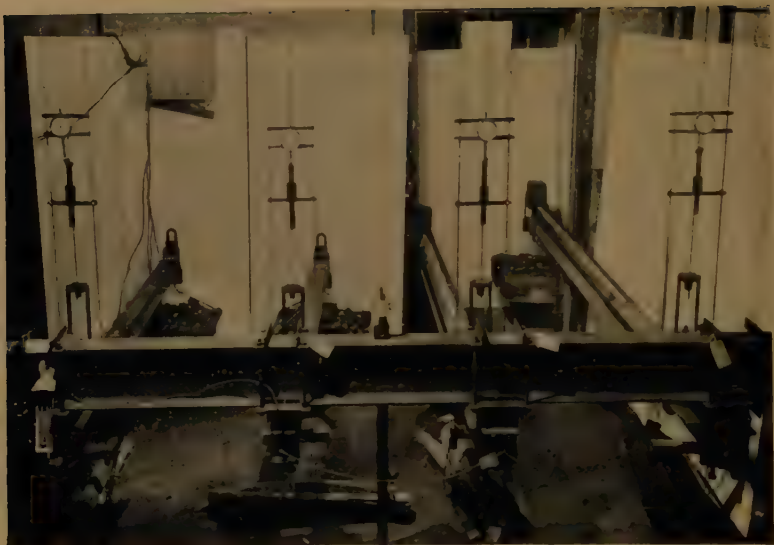


FULCRUM OF LOADING LEVER ATTACHED TO
LOWER ANCHORAGE BEAM



CLOSE-UP OF DIAL AND VERNIER
SLIDE GAUGES

Fig. 5



GENERAL VIEW OF TEST RIG SHOWING FOUR WIRES UNDER TEST

Fig. 6



6-TO-1 LOADING LEVER WITH TEST IN PROGRESS ON 0.104-INCH-DIAMETER WIRE,
SHOWING VERNIER SLIDE GAUGE AND THE DIAL GAUGE IN POSITION

such levers were provided, two with a leverage of 6 to 1 for the 0.104-inch-diameter wires and two with a leverage of 8 to 1 for the 0.2-inch- and 0.276-inch-diameter wires, so that four specimens could be tested simultaneously (*Fig. 5*). This device conveniently reduced and facilitated the application of the loading weight, increased the sensitivity of the early observations by magnifying the small movements of the test wire, and was self-compensating for changes in the length of the wire or the distance between top and bottom anchorages.

The observation of wire movement in the constant-length test is effected first by means of an adjustable post resting on the ground and carrying the fixed contact of an electrical device which lights a lamp when the loading lever sinks on to the contact (see *Fig. 2*). Reduction of the load permits the wire to shorten slightly and so raise the lever, break the contact, and put out the light. The movements involved are only of the order of 0.001 inch. This apparatus is used only during the first hour or so when the creep is rapid and temperature variations are small enough to be ignored. Thereafter, observation is effected through either a vernier slide gauge or a dial gauge, or both, attached to a pair of lightly loaded wires hanging on either side of the test wire, and having their moving parts attached to or bearing on a trigger attached to the test wire. (See *Figs 4 and 6*.) By these means a constant length "zero" is established and the slight lengthening of the test wire caused by creep is observable and is corrected by removal of known weights from the end of the lever until the gauges return to zero and the constant length is re-established.

The starting load is applied by means of an independently supported wire-rope winch. The time taken to apply the load is 1 to 5 minutes, and the length datum is established as soon as the total load is carried by the wire.

For constant-load tests the same apparatus can be used, but instead of reducing load to bring the gauges back to "zero," the load remains unchanged and the gauges are read progressively. For this condition the electrical device is not needed.

The designed maximum loadings were 3,000 lb. per wire on the 0.104-inch-diameter wire and 11,000 lb. per wire on the 0.2-inch and 0.276-inch-diameter wires.

The apparatus, with the exception of the dial gauges, was designed and made up in the workshops of the Unit from stock materials.

Temperature Variations

By means of two lightly loaded reference wires hung from the top anchorage beam in the tower, one on either side of the test wire, and to which the static members of the vernier and dial gauges were attached, all variations in the length of the test wire due to temperature were fully compensated. These reference wires proved satisfactory for light loads, but for the heavier loadings and for all the larger diameter wires they were

suspended from the test wire itself just below the top anchorage, thus eliminating errors resulting from the "pull-in" of the top grip.

Temperature variations in the height of the tower caused both the test and reference wires to rise and fall equally relative to the bottom anchorage beam. In other words, the angle of the loading beam with the horizontal plane changed with these temperature variations. This did not affect the loading since the ratio of the moment arms on the beam is independent of its angle to the horizontal. The gauge zeros were of course also unaffected. Both these movements, however, precluded the use of the electrical device as a creep indicator since it was resting on the floor of the tower. It was therefore used only during the first hour of the test when temperature variations were insignificant.

Disturbance of the Apparatus

It was found that the application of load to the second and subsequent test wires disturbed the wires already under test owing to the fact that the bottom anchorage was common to all, and vibrations were transmitted through it. In one or two instances, where a pull-out of a subsequent test wire occurred during setting-up, the disturbance was great enough to upset completely the other tests in progress, and they had to be abandoned and a fresh start made. Minor disturbances were dealt with by re-establishing the length zero on the tests in progress. This difficulty could not be overcome without providing separate bottom anchorages for each wire.

Frequency of Observations

In order to ascertain when the frequency of observations could safely be reduced, a rough graph of the early readings was plotted with large load and time scales. The graph showed when the creep rate had fallen off considerably, and after several trials with readings at intervals of 5 minutes, it was found that the first hour amply covered this phenomenon for the smaller wire sizes.

THE TESTS

Preparation of the Apparatus

The procedure for setting-up a test was as follows.

The load required at the end of the loading beam to produce the specified load on the test wire was weighed out and placed on or attached to the loading tray, which was supported by the winch. Some of this required weight was in the form of loose known weights ranging from 2 lb. to 1 oz., placed in the tray for easy adjustment of load during the test. The fulcrum end of the loading beam was supported by a screw jack from the floor. The loaded end of the beam was then raised by means of the winch and held at such a height that, on transfer of the load to the test wire, the loading beam would be approximately horizontal.

The test length of wire was then threaded through the floors of the tower, anchored at the top support, pulled as tight as possible by hand and gripped at the bottom anchorage. Safety stops were then attached to the wire at the intermediate floor levels. The fulcrum knife-edge was next raised into its correct position by means of the adjustable screw link incorporated in the lower anchorage. The screw jack was then lowered clear of the beam, thus transferring the weight of the fulcrum end of the loading beam to the test wire. The electrical contact device was then checked for correct operation and the post carrying the fixed contact placed roughly in position beneath the loading beam. The vernier slide gauge was next clamped in position on the reference wires and the test wire in such a position as to allow approximately for the expected extension of the wire when loaded. The screw jack was then placed under, but not in contact with, the beam near the wire anchorage, to act as a safety stop in the event of a pull-out at a grip or a broken wire during loading.

The apparatus was then ready for the test to commence.

TEST PROCEDURE

Load Application

One operator carefully and slowly lowered away on the winch, in this way gradually loading the test wire until the winch stopped unwinding, which indicated that the whole load was carried by the test wire; the winch brake was then applied, thus preventing any further lengthening of the wire owing to creep. He then quickly adjusted the lighting contact so that the warning light was just "on." Simultaneously, a second operator read the vernier slide gauge and started a stopwatch, thus establishing the datum for the constant length and the time zero. The winch was then removed, and the wire was set free for creep extension.

Frequency of Load Adjustment

After an interval of 5 minutes, the first observation was made by removing weights from the loading tray until the light just went out, and recording the loss in weight. Subsequent observations were taken at intervals of 5 or 10 minutes for the first hour. A check was made by replacing a 2-oz. weight on the tray. This would cause the light to come on again if the observation had been correct. Check readings were also taken on the vernier gauge to ensure that the test length had not changed because of slip in the top wire anchorages.

After the first hour a dial gauge was set between the reference wires and the test wire (*Fig. 4*), and its zero position was established as the indicator light went out on the next load adjustment. Check readings were taken on the vernier gauge but the dial gauge was relied upon to establish the length datum for the remainder of the test. The electrical indicator was then removed. Observations of the reduction in load required to bring the dial gauge back to its "zero" were then taken every

hour for the next 4 hours and subsequently at intervals of 24 hours for the remainder of the test, the total duration being 1,000 hours.

ACCURACY OF THE APPARATUS

The electrical device was found to be extremely sensitive in that the removal of a 2-oz. weight from the loading scale was sufficient to put out the warning light, and conversely its addition would put on the light. This meant that with a lever ratio of 8 to 1 a change in the loading of the wire of 1 lb. was detectable even when the total load on the wire was 5,960 lb. The length gauges were perhaps not quite so sensitive although the vernier could be read to within 0.005 inch and the dial gauge to 0.001 inch, which represented load changes on the 0.104-inch-diameter wire of 2.75 lb. and 0.55 lb. respectively.

As mentioned above, possible errors owing to movement of the load as creep in the wire developed, movement of the fulcrum resulting from deflexion of the support when other specimens were loaded, and to temperature, were automatically eliminated by the design of the apparatus. The error owing to slip in the top anchorage was not compensated in the earlier tests but was later eliminated as previously described. The top anchorage of the 0.104-inch-diameter wire did not suffer from this defect.

RESULTS

Creep tests were carried out on the wires shown in Table 1.

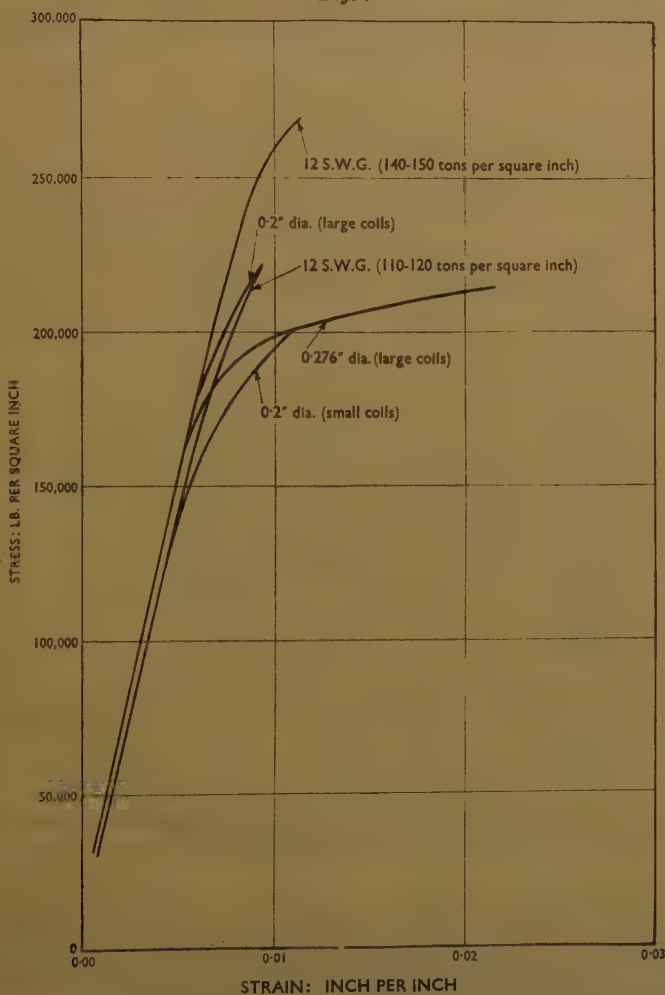
TABLE 1.—WIRES TESTED

Wire	Ultimate strength :		Diameter of coils	
	tons per square inch	lb. per square inch	Coiled	Free
12 S.W.G.	110-120	246,400-268,800	3'-0"	3'-0"
12 S.W.G.	140-150	313,600-336,000	2'-0"	3'-0"
0.2-inch diameter . . .	100-110	224,000-266,400	2'-9"	2'-9"
0.2-inch diameter . . .	100-110	224,000-266,400	8'-0"	straight
0.276-inch diameter . .	100-110	224,000-266,400	8'-0"	straight

A range of initial stresses was employed in an attempt to arrive at a general curve showing the relationship between creep loss and applied load, which might be applicable to wires of other ultimate strengths. The range of the initial applied stress was 20-80 per cent of the ultimate strength of the wires. It will be noted that two variables have been investigated—diameter and ultimate tensile strength. To investigate the

first, three different diameters were used with approximately equal ultimate tensile strengths, and to investigate the latter, two wires of the same diameter, but of different ultimate strengths, were employed.

Fig. 7



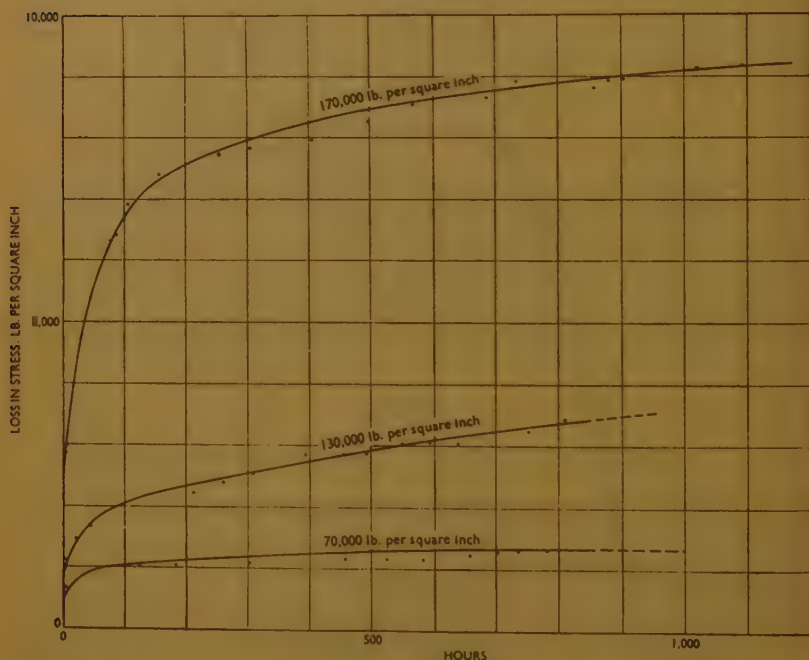
STRESS/STRAIN CURVES OF HIGH-TENSILE WIRES USED IN TESTS

Stress/strain curves of the wires before and after test were obtained. In general, no systematic difference was observed in the properties of the wires. The values of the 0.1-per-cent proof stress and of the ultimate stress for the different wires are given in Table 2 and typical stress/strain curves for the different wires are shown in *Fig. 7*.

The method of testing gives the initial load and the subsequent loads at different intervals of time necessary to maintain the wire at constant length. From these figures can be deduced the initial stress and the loss in stress which occurs with time.

The tests were carried out over a relatively short period of time—1,000 hours; it is desirable, however, to gain some idea of the creep which may

Fig. 8



CREEP OF 0.2-INCH-DIAMETER HIGH-TENSILE STEEL WIRE (100-110 TONS PER SQUARE INCH ULTIMATE), 8-FOOT COILS

occur over a larger period of time. In an attempt to give some assessment of this loss, the results obtained have been plotted on a logarithmic time basis as well as on a normal time basis. In the former case, although the curve is not linear, a linear extrapolation has been made.

A typical set of curves showing loss in stress against time is given in *Fig. 8* for 0.2-inch-diameter wire (large coils), and sets of curves for all the wires tested showing the loss in stress plotted against log-time are shown in *Figs 9, 10, 11, and 12*. The straight lines drawn in *Figs 9, 10, and 11* do not necessarily represent the behaviour of the wire over the whole duration of loading, but do so over the greater portion of the time. The

form of the curve is probably different, and lines passing through the observed points have been drawn in on *Fig. 12*.

TABLE 2.—PROPERTIES OF HIGH-TENSILE STEEL WIRE

Test No.	Diameter of wire : inch	Initial stress : lb. per sq. inch	Before test		After test	
			0.1-per-cent proof stress : lb. per sq. inch	Ultimate stress : lb. per sq. inch	0.1-per-cent proof stress : lb. per sq. inch	Ultimate stress : lb. per sq. inch
1	0.104 (12 S.W.G.)	52,500	177,000	260,000	171,500	258,000
2	"	105,000	179,000	256,000	178,000	256,000
3	"	157,000	177,000	260,000	190,000	259,000
4	"	210,000	179,000	256,000	213,000	262,000
5	"	70,000	224,000	308,000	224,000	310,000
6	"	146,000	211,000	316,000	226,000	319,000
7	"	175,000	211,000	311,000	235,000	318,000
8	"	204,000	244,000	320,000	202,000	317,000
9	"	232,000	244,000	320,000	206,000	320,000
10	"	255,000	224,000	308,000	231,000	300,000
11	0.20	70,000	125,000	225,000	181,000	254,000
12	"	120,000	168,000	251,000	161,000	247,000
13	"	130,000	161,000	248,000	142,000	246,000
14	"	142,000	161,000	238,000	170,000	241,000
15	"	152,000	161,000	238,000	166,000	237,000
16	"	190,000	—	—	197,000	244,500
17	"	70,000	212,000	251,000	214,000	245,000
18	"	130,000	212,000	251,000	212,000	247,000
19	"	170,000	212,000	251,000	211,000	244,500
20	0.276	67,000	187,000	229,000	180,000	222,500
21	"	101,000	187,000	229,000	180,000	222,500
22	"	130,000	187,000	229,000	182,500	126,000
23	"	168,000	187,000	225,000	187,000	225,000

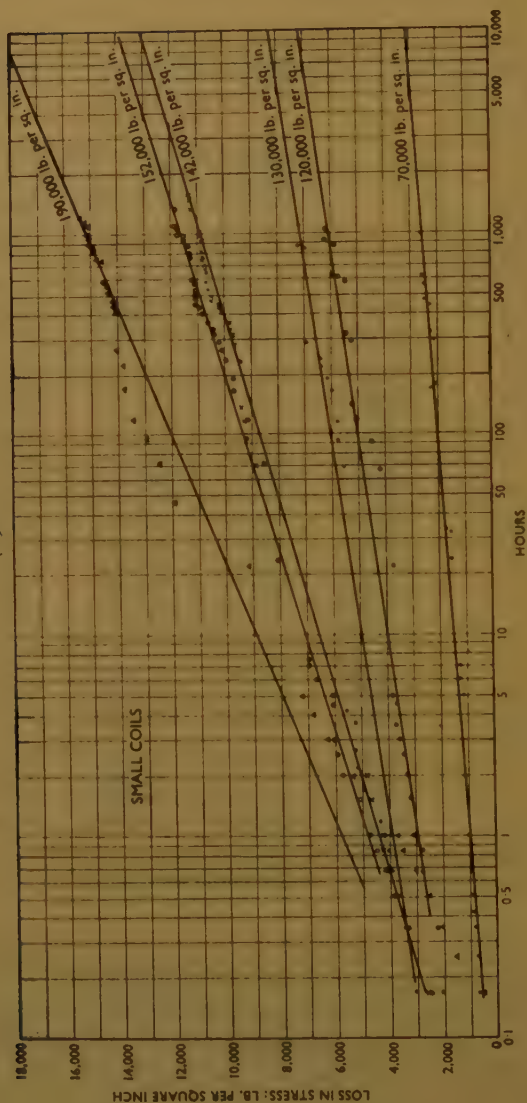
NOTE.—Tests Nos. 17–23 were carried out on wire supplied in large-diameter coils.

The loss in stress of a loaded wire at constant length is presumably dependent upon the applied stress and some property of the wire. This latter property is likely to be dependent upon the crystalline structure of the wire and upon the internal stresses present in a drawn wire. These in turn are a function of the amount of drawing which takes place, and any work- or age-hardening which may occur. Some of the results of these processes are reflected in the stress/strain curve of the particular wire, both in its shape and also in the value of its ultimate strength and elongation at failure.

Since the only information which is readily available is the stress/strain

Figs 9

(a)



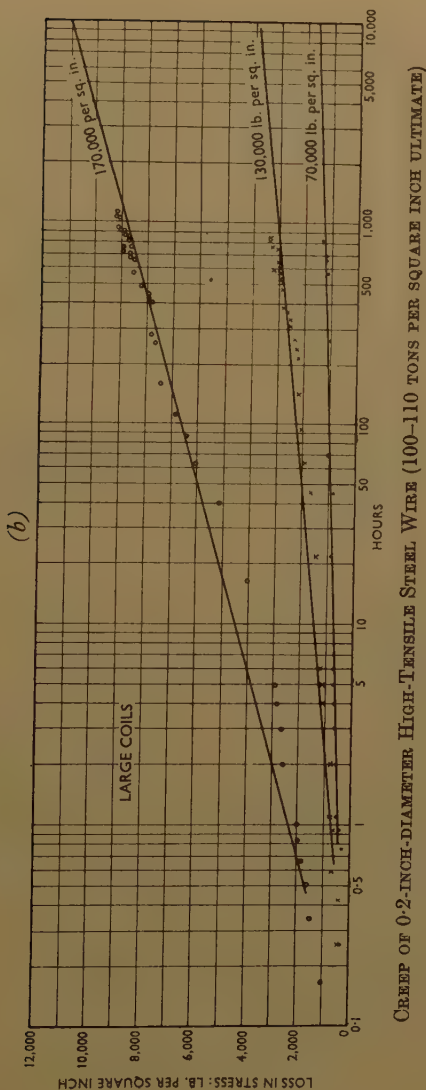
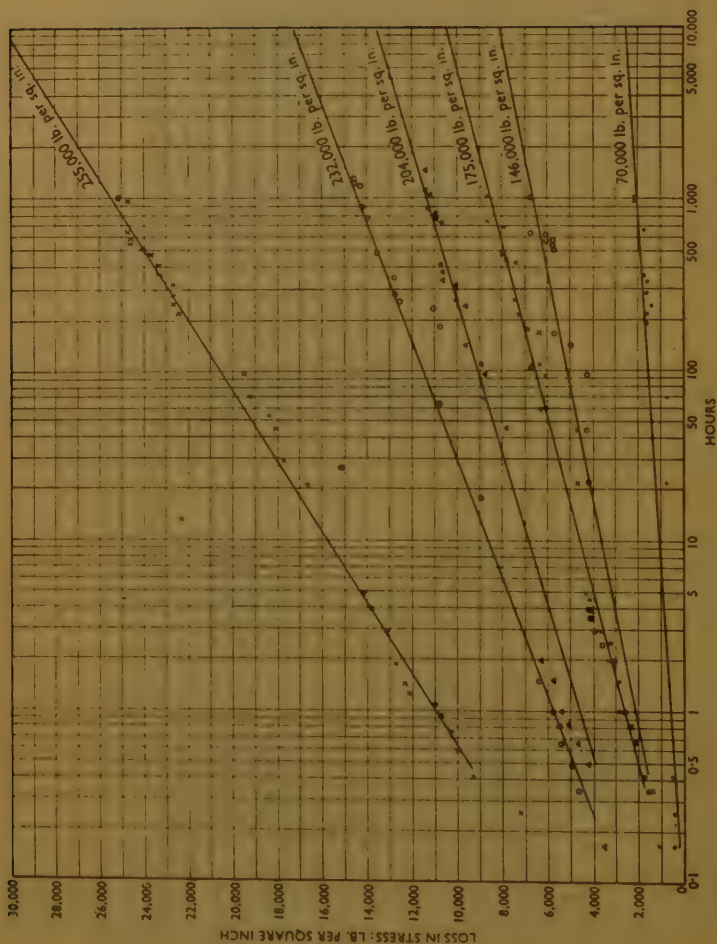
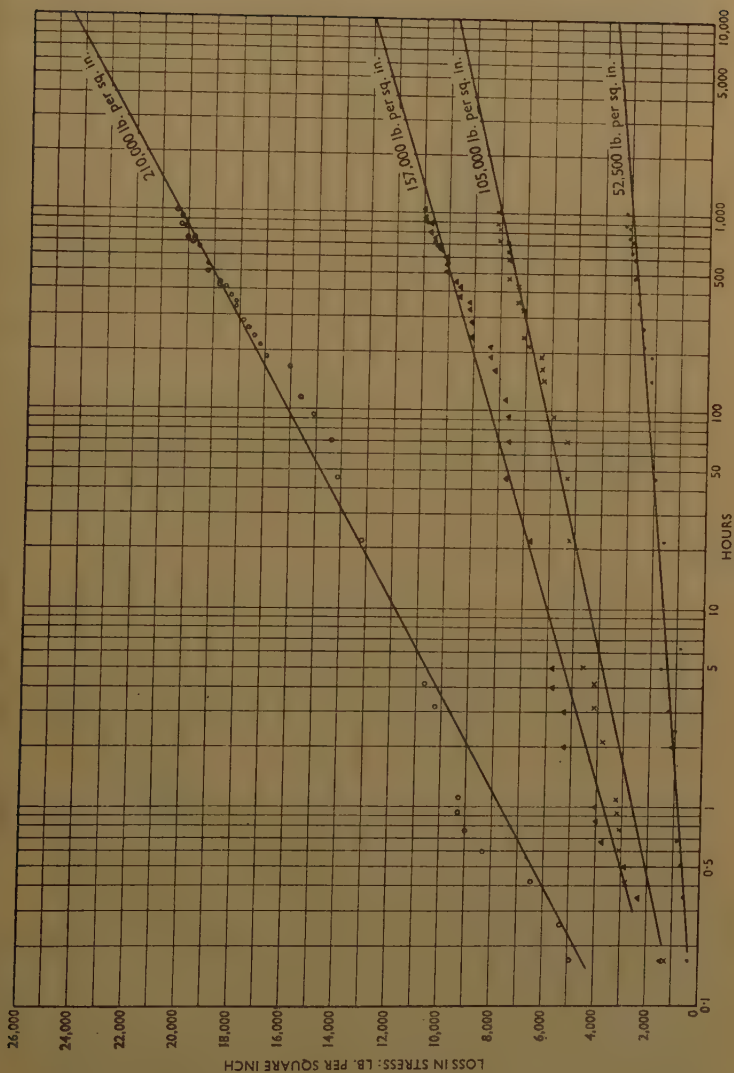


Fig. 10



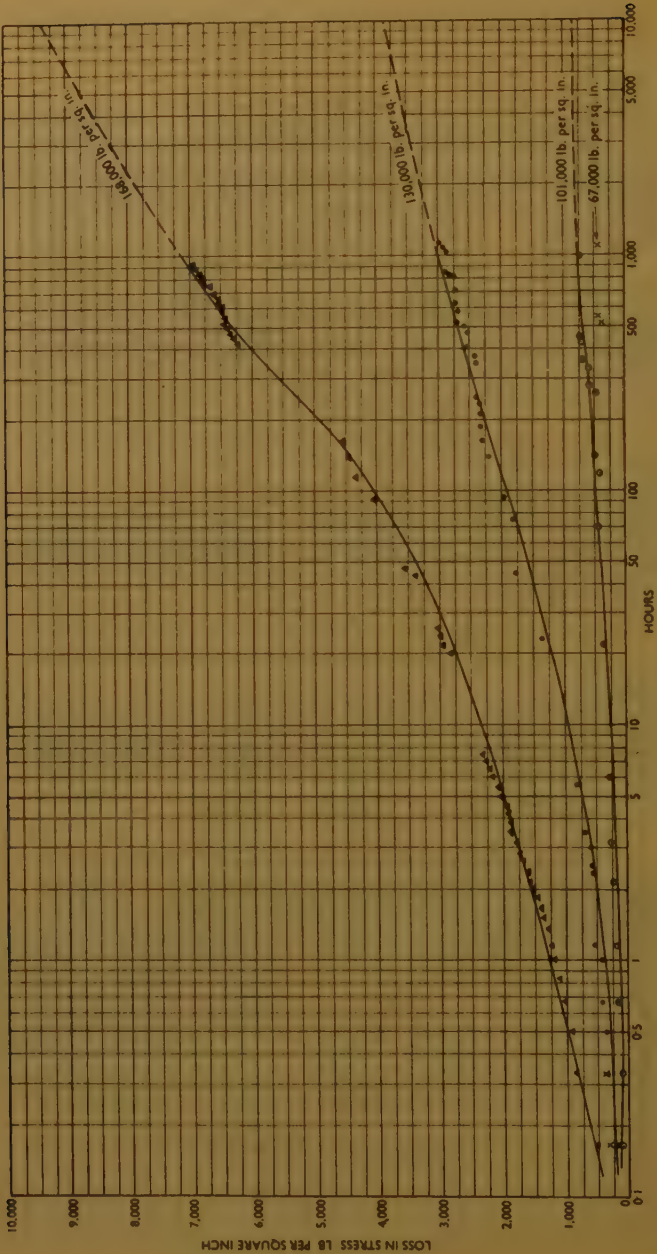
CREEP OF 12 s.w.g. HIGH-TENSILE STEEL WIRE (140-150 TONS PER SQUARE INCH ULTIMATE)

Fig. 11



CREEP OF 12 S.W.G. HIGH-TENSILE STEEL WIRE (110-120 TONS PHI SQUARE INCH ULTIMATE)

Fig. 12

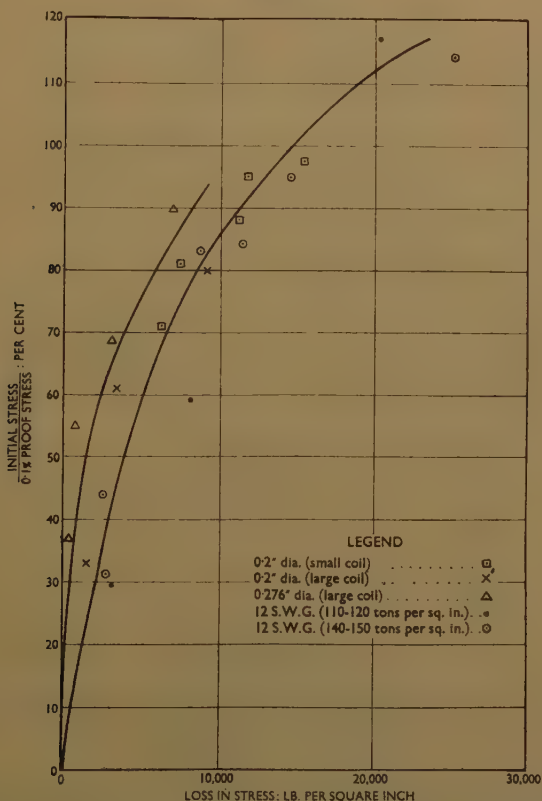


CREEP OF 0.276-INCH-DIAMETER HIGH-TENSILE WIRE (100-110 TONS PER SQUARE INCH ULTIMATE)

curve, the creep loss in the steel has been considered in relation to the ratio of the applied stress to the ultimate stress, and also to the 0.1-per-cent proof stress, although the latter is a completely arbitrary quantity.

Table 3 gives the losses in stress at various times and the ratio of the applied stress to the ultimate and 0.1-per-cent proof stress of the wire.

Fig. 13



VARIATION OF LOSS IN STRESS AT 1,000 HOURS WITH $\frac{\text{INITIAL STRESS}}{\text{0.1\% PROOF STRESS}}$ FOR
 HIGH-TENSILE STEEL WIRE

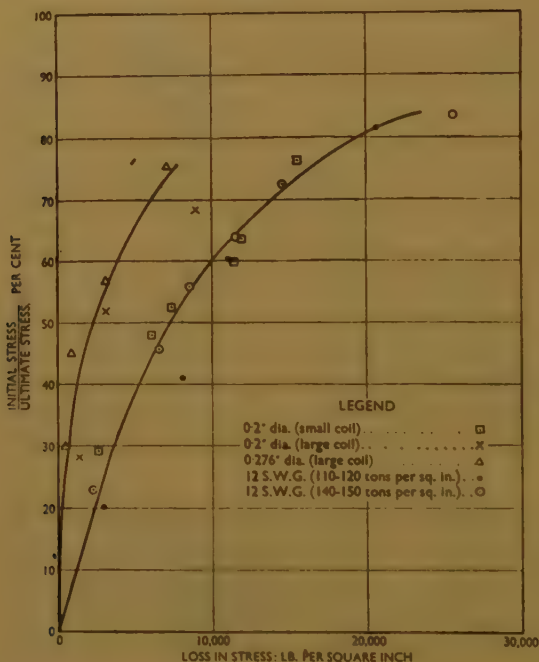
The values of the loss in stress at 1 hour and 10 hours have been obtained by interpolation, and at 10,000 hours by extrapolation.

The loss in stress at 1,000 hours has been plotted against the ratio of the initial stress to the 0.1-per-cent proof stress in Fig. 13, and against the ratio of the initial stress to ultimate stress in Fig. 14. Two curves have been drawn, one for all the wires which come normally through the die

and are wound on to small coils, and the second where the wire is subsequently straightened and re-wound on to large-diameter coils. The latter shows a markedly smaller amount of creep, which is not altogether accounted for by a high proof stress.

Although it would be desirable to plot non-dimensional quantities, the fit of the results is not so good, as will be seen from *Fig. 15*, where the ratio

Fig. 14



VARIATION OF LOSS IN STRESS AT 1,000 HOURS WITH $\frac{\text{INITIAL STRESS}}{\text{ULTIMATE STRESS}}$ FOR HIGH-TENSILE STEEL WIRES

of the initial stress to ultimate stress is plotted against the ratio of the loss of stress to initial stress.

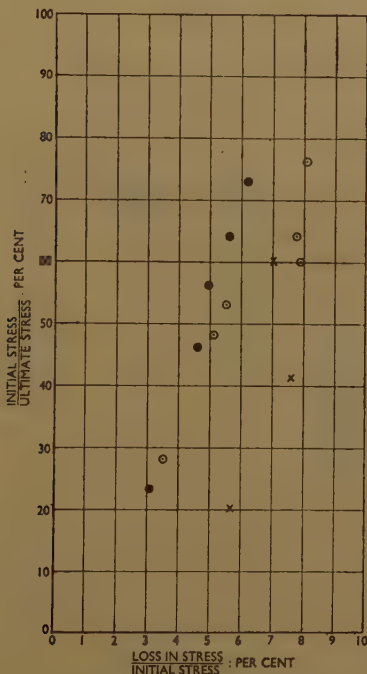
Fig. 16 shows the approximate losses of stress at different times as a function of the applied stress to the ultimate stress for wire supplied in small-diameter coils. *Fig. 17* shows the same for wire supplied in large-diameter coils.

COMPARISON OF THE ABOVE RESULTS WITH OTHER PUBLISHED DATA

In the introduction to this Paper other work in the same field was noted. This work will now be examined in a little more detail.

(a) *Work of Professor Magnel*²

The characteristics of the 5-mm.- and 7-mm.-diameter wires used in Professor Magnel's tests, together with their equivalent loss of stress (measured under conditions of constant load) are shown in Table 4.

Fig. 16

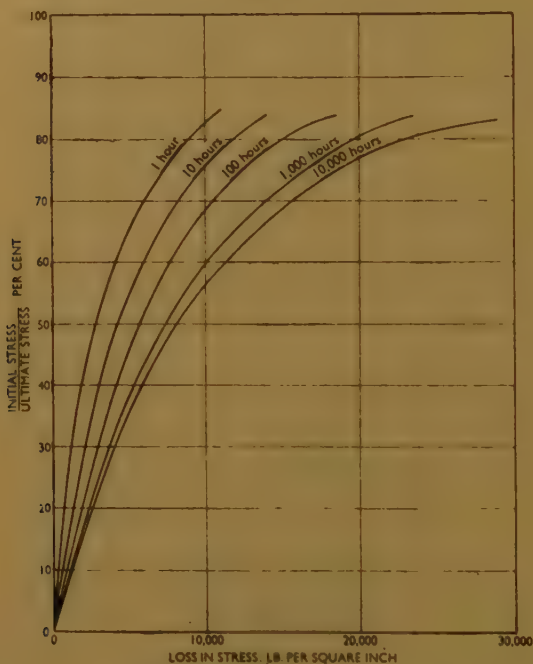
LEGEND
 Small coils, 12 S.W.G. (110-120 tons per sq. in.) - x
 Small coils, 12 S.W.G. (140-150 tons per sq. in.) - o
 Small coils, 0.2" dia. (100-110 tons per sq. in.) - ●

VARIATION OF $\frac{\text{LOSS IN STRESS}}{\text{INITIAL STRESS}}$ WITH $\frac{\text{INITIAL STRESS}}{\text{ULTIMATE STRESS}}$ AT 1,000 HOURS FOR
 HIGH-TENSILE STEEL WIRES

The equivalent loss of stress has been obtained by multiplying the increase of strain by the ratio of the stress to strain at the stress employed. It is realized that there is no real justification for this procedure. A better way might be to use a factor obtained by dividing the loss of stress obtained in constant-length experiments by the increase in strain in constant-stress experiments. This can only be done in this case for a time of 300 hours when the "modulus" obtained in this way is 22×10^6 lb. per square inch.

The tests noted here are not strictly comparable with those described in this Paper since the full load took $2\frac{1}{2}$ minutes to apply and the wires were tested under conditions of constant load. Two tests were carried out under constant-length conditions for a short duration but these are not discussed here. It will be seen that the equivalent creep losses in the more highly stressed wires as well as wire 5-I are far in excess of those

Fig. 16



VARIATION OF LOSS IN STRESS WITH $\frac{\text{INITIAL STRESS}}{\text{ULTIMATE STRESS}}$ AT DIFFERENT TIMES FOR HIGH-TENSILE STEEL WIRES (SMALL COILS)

observed in the tests given in this Paper, whilst the equivalent creep loss in the more lightly loaded specimens is less. This may be caused by the different quality of steel, and also it is likely that constant-length and constant-load experiments are not strictly comparable.

(b) *Work of the Swiss Federal Laboratory*³

Certain tests are recorded in the Report on Prestressed Concrete issued by the Swiss Federal Laboratory. In addition, an expression is given which may be used when experimental results are not available.

TABLE 3.—CREEP LOSSES IN GIVEN TIMES

Test No.	Ratio of applied stress to :		Loss in stress : lb. per square inch				
	0.1-per-cent proof stress	Ultimate strength	1 hour	10 hours	100 hours	1,000 hours	10,000 hours (estimated)
1	0.29	0.20	900	1,500	2,250	3,000	3,600
2	0.59	0.41	3,100	4,500	6,100	8,000	9,750
3	0.89	0.60	4,000	6,000	8,400	11,000	13,000
4	1.17	0.82	9,000	12,000	16,000	20,500	24,500
5	0.31	0.23	400	500	1,500	2,200	2,500
6	0.70	0.46	2,500	4,000	5,000	6,700	8,000
7	0.83	0.56	2,500	4,500	6,500	8,600	10,500
8	0.84	0.64	5,400	7,500	9,000	11,500	13,500
9	0.95	0.73	5,750	8,600	11,500	14,500	17,250
10	1.14	0.83	11,000	15,500	19,500	25,000	30,250
11	0.44	0.28	1,000	1,500	2,000	2,480	3,000
12	0.71	0.48	3,000	4,000	5,000	6,200	7,100
13	0.81	0.53	4,000	5,000	6,000	7,200	8,200
14	0.88	0.60	4,200	6,500	9,000	11,200	13,000
15	0.95	0.64	4,750	7,000	9,300	11,800	14,000
16	0.97	0.76	—	8,000	13,000	15,400	18,200
17	0.33	0.28	500	750	1,000	1,300	1,600
18	0.61	0.52	750	1,500	2,300	3,200	4,000
19	0.80	0.68	2,000	3,500	6,700	9,000	11,000
20	0.37	0.30	370	370	370	370	370
21	0.55	0.45	180	270	404	720	720
22	0.69	0.57	450	800	2,000	3,000	3,800
23	0.90	0.75	1,200	2,500	4,100	7,000	9,300

TABLE 4

	Wire No.		
	5-I	5-III-B	7-I-B
0.1-per-cent proof stress .	143,000*		
Ultimate strength . . .	216,000	245,000	207,000
Initial stress . . .	123,000	188,000	150,000
Loss in stress in 1,000 hours .	17,800	26,200	19,500
Initial stress . . .		147,000	125,000
Loss in stress in 500 hours .		3,000	1,300

* All values in lb. per square inch.

The tests were carried out on 3.2-mm.-diameter Swedish wire having the following properties :—

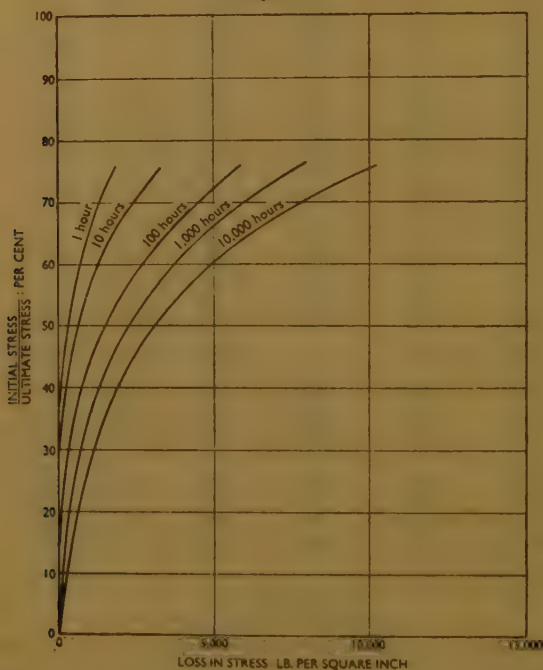
Ultimate tensile strength = 279,000 lb. per square inch,

0.2-per-cent proof stress = 240,000 „ „ „ „

0.1-per-cent proof stress = 228,000 „ „ „ „ (estimated).

The creep losses measured at constant length at stresses of 213,000, 185,000, 156,000, and 107,000 lb. per square inch were 20,000, 9,300,

Fig. 17



VARIAION OF LOSS IN STRESS WITH $\frac{\text{INITIAL STRESS}}{\text{ULTIMATE STRESS}}$ AT DIFFERENT TIMES FOR HIGH-TENSILE STEEL WIRES (LARGE COILS)

4,300, and 1,500 lb. per square inch. These were recorded after different periods of loading when it was judged that creep had finished.

The results of these tests are plotted in *Fig. 18*, where the curve from *Fig. 13* is also reproduced to provide comparison with the results given in this Paper. The points follow the general shape of the curve except at high stresses where the creep loss probably depends to a large extent on the point at which the "turn-over" occurs in the stress/strain curve.

The Swiss investigators also suggest that an expression similar to the following fits the experimental data :

$$e_k = \frac{1}{10} \left(\frac{\sigma}{0.45\sigma_s} - 1 \right)^2$$

where e_k denotes the increase in strain which takes place at constant load, expressed as a percentage ;

σ denotes the applied stress ; and

σ_s denotes the 0.2-per-cent proof stress.

This expression assumes that no creep occurs in the steel when it is stressed to less than $0.45\sigma_s$; it seems to fit the experimental data given for creep at constant load reasonably well. It does not fit the data obtained at constant length.

The same report gives data on the increase in elongation of the same steel at constant load and this would seem to be equivalent to about $1\frac{1}{2}$ to 2 times the creep loss at constant length. This may be the explanation of the high creep losses observed by Professor Magnel.

(c) *Work of the " Laboratoires du Bâtiment et des Travaux Publics "* ⁴

G. Dawance, in his report on the creep of steel, describes a method of measuring the stress in wires by measuring their frequency, and gives a number of results obtained using this method. The tests were carried out at constant length.

The wires used in these tests were 2.5 and 5.0 mm. in diameter and had the properties listed below.

Diameter : mm.	0.1-per-cent proof stress : lb. per sq. inch	0.2-per-cent proof stress : lb. per sq. inch	Ultimate strength : lb. per sq. inch
2.5	215,000	227,000	285,000
5.0	175,000	184,000	225,000

The tests on the 5.0-mm.-diameter wires were of relatively short durations—200 to 300 hours ; the creep losses obtained in these times are given in Table 5.

The tests on the 5.0-mm.-diameter wire do not agree very well with the results recorded in this Paper, but a considerable scatter is always observed in the first 100 hours.

The tests on the 2.5 mm. wire were of long duration and show, contrary to the observations of Professor Magnel and the Swiss Laboratories, that the creep goes on indefinitely although at a very slow rate.

The results of the tests on the 2.5-mm.-diameter wire are shown in Table 6 and plotted in *Fig. 18*, where they will be seen to follow the shape of curve recorded in the present Paper.

TABLE 5.—CREEP TESTS ON 5-MM.-DIAMETER WIRE

Ratio of initial stress to 0.1-per-cent proof stress : per cent	Ratio of initial stress to ultimate stress : per cent	Loss of stress in 1 hour : lb. per sq. inch	Loss in stress in 100 hours : lb. per sq. inch
65	50	1,700	3,500
95	67	2,100	5,000
104	80	3,700	8,900
116	90	5,700	15,600
122	94	8,500	16,500

TABLE 6.—CREEP TESTS ON 2.5-MM.-DIAMETER WIRE

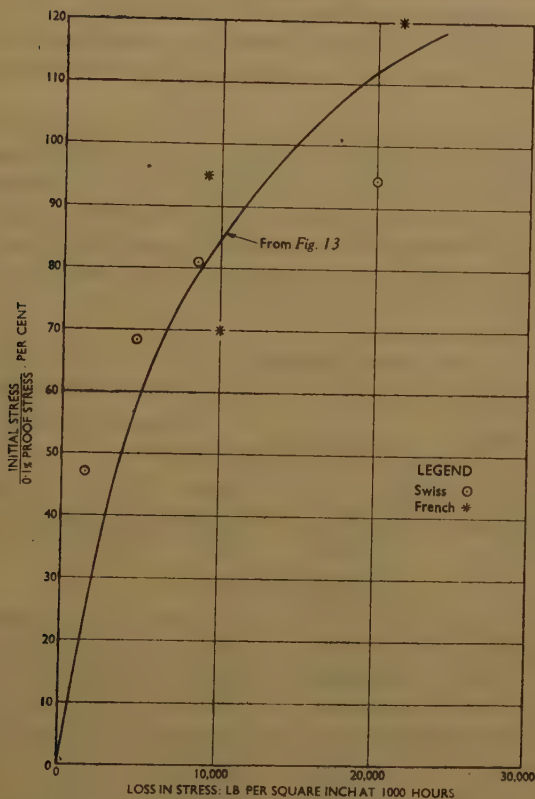
Ratio of initial stress to 0.1-per-cent proof stress : per cent	Ratio of initial stress to ultimate stress : per cent	Loss of stress : lb. per sq. inch		
		100 hours	10,000 hours	20,000 hours
70	53	10,000	14,000	—
95	71	9,200	17,100	18,500
119	90	21,500	31,500	32,600

The recorded work of de Strycker ⁵ is of an exploratory nature. In it he has investigated the effect of initially overstressing the wire by 10 per cent for a 2-minute duration, and confirms that there is a reduction in the creep loss (measured at constant load). He shows, however, that the rate of creep loss is subsequently the same so that the percentage reduction of the creep loss is diminished with time. The reduction of creep loss in the wire he tested was not nearly so great as that recorded by Professor Magnel.

THE APPLICATION OF THE RESULTS TO PRESTRESSED CONCRETE

The preceding work has shown that the phenomenon of creep of steel should not be ignored in the majority of cases when designing or erecting prestressed concrete structures. The creep loss in the wire which is wound straight into small coils from the die is large, and allowance should be made for it. The creep loss of wire straightened and re-wound into large coils is very much smaller, at any rate during the period of time of the test, and further developments by the wire-drawing industry will no doubt result in wire with even smaller creep losses, obviating the need to allow for it, but in the meantime an allowance should be made in both cases for this loss.

Fig. 18



COMPARISON OF CREEP LOSSES OBSERVED IN FOREIGN TESTS WITH THOSE IN THE PAPER

The losses recorded in this Paper are likely to be greater than those recorded on an actual job because :—

- (a) It takes an appreciable time when carrying out the prestressing operation to stress the wires, and while this is being done the creep loss is taking place.
- (b) The concrete is shortening both elastically and plastically, so that the stress in the wires is continuously reduced.

Of these two reasons, (a) is likely to be more important than (b). When prestressing is being carried out the pressures recorded on the gauges on the jacks and the elongation of the wire are both used to assess the pre-stress in the wires. In general, the elongation is calculated from the known stress/strain relationship of the wire ; if any advantage is to be " claimed " of the reduction in creep loss by virtue of the time it takes to stress the wires,

then the stress/strain relationship should be recorded under approximately the same rate of loading as that achieved on the job.

In the case of (b), a great deal of relief is not to be expected, since the greater part of the creep loss occurs before any appreciable shortening of the beam takes place.

Nevertheless, for the above two reasons it may be sufficient to assume that the creep loss in 10,000 hours is the maximum loss which will occur. It should, however, be borne in mind that until further results over longer periods of time are available, extrapolation to 10,000 hours might not be a safe procedure. On this assumption the percentage creep losses for different wires for various initial stresses may be tabulated as shown in Table 7.

TABLE 7

Ultimate strength of wire : tons per sq. inch	Initial stress : lb. per sq. inch	Percentage creep loss		
		1 hour	100 hours	10,000 hours
100-110 (small coils)	120,000	2.3	4.8	6.8
	130,000	2.6	5.1	7.4
	140,000	2.9	5.5	8.2
	150,000	3.1	5.6	8.7
140-150 (small coils)	140,000	1.5	3.2	4.9
	180,000	2.0	3.7	5.6
	210,000	2.4	4.3	6.6
	240,000	2.9	5.0	7.5
100-110 (large coils)	120,000	0.33	1.4	2.8
	130,000	0.46	1.8	3.5
	140,000	0.57	2.2	3.9
	150,000	0.74	2.5	4.5

These losses are not inordinately high in the majority of cases, whilst they are very small in the case of the wire in large coils. The percentage loss in 1 hour in the latter wire is small compared with the estimated loss in 10,000 hours, and in this it differs from the wires wound into small coils where the loss is about one-third.

For wire in small coils it is suggested that the losses may be effectively countered by permanently over-stressing the wires initially. Where, in post-tensioned and in pre-tensioned work, the wires are stressed progressively a few at a time, it is not practicable to over-stress the wires for a few minutes, and then to reduce the stress ; the process takes too long. Since, however, the greater part of the creep loss occurs in the first few days, and the major part of this in the first hour, it is considered that the best method of eliminating the effect of creep in post-tensioned work is to over-stress the wires initially by about one-half of the total anticipated creep loss.

The remaining loss must be allowed for in the overall loss assumed in the calculations. If, however, the stresses in the concrete are not critical, over-stressing by the full amount of anticipated creep loss might be considered, when only a small allowance need be made in the overall loss assumed.

In pre-tensioned work, the amount of initial over-stressing will depend upon the length of time elapsing between stretching the wires and releasing them. If this time is several days, then the wires may be over-stressed initially by the amount that they would lose by creep in that time. If steam heating is employed and the time between stretching and releasing is short, then the matter must be considered in greater detail, but it will probably be safe to make the same assumptions as for post-tensioned work.

When wire in large coils is used, the creep losses (in these tests) are much smaller and it will probably be sufficient to make allowance in the calculations for the creep of steel without over-stressing the wire on the job.

FURTHER RESEARCH

Further research on the whole question, including the effects of over-stressing, is now being carried out by the Building Research Station.

ACKNOWLEDGEMENTS

The Authors are indebted to the Director-General of Works of the Ministry of Works, and the Director of the Building Research Station, for permission to publish this Paper, and to those members of the staff at the Field Test Unit, Thatched Barn, Barnet, who were responsible for the detailed design of the apparatus and for carrying out the experimental work.

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The Paper is accompanied by five photographs and thirteen sheets of diagrams, from which the half-tone page plates and the Figures in the text have been prepared.

Discussion

The Authors introduced the Paper with the aid of a series of lantern slides.

Mr C. L. Champion observed that the Authors had made a valuable contribution to the very limited data on the creep of steel at normal temperatures. The data were scanty because, of course, the problem was a very recent one. A great deal of work had been done on the creep of steel at high temperatures, and that was important in a number of fields, but at the stresses which had normally been used it had been considered that the creep at ordinary temperatures was negligible. It was the introduction of exceptionally high stresses for prestressed concrete which had led to the creep problem.

The civil engineer was concerned with structures which were to last for a very long period of time. The experiments described by the Authors had been carried out over the limited period of 1,000 hours, and Mr Champion wished to offer a little friendly criticism of the interpretation of the results. Extrapolation was always a dangerous proceeding, and was particularly deceitful when carried out on logarithmic paper, where it might lead to all sorts of conclusions. It was justified only if certain knowledge was available of the physical law behind the phenomena which were being studied, and in the present instance no argument had been advanced to show why that law should be a logarithmic one. Certain writers on creep had suggested that the normal curve of creep could be divided into three stages. In the first, the creep increased very rapidly; then there was a stage in which, over a prolonged period of time, the rate of creep was nearly uniform; and finally there was a stage when that rate increased until failure occurred. Mr Champion's first slide was a simple graph illustrating those three stages, and he explained that it referred to creep at constant load. In practice, the matter was very complicated, because the stretch of the steel altered its state, and such a curve was rarely, if ever, encountered in practice. In fact, in many cases where the creep of steel had been measured at moderate stresses, the rate of creep had been diminishing after long periods, and it was not certain whether a limiting value of total creep would be reached or whether, after an immensely long period, the rate of creep would increase again. The relaxation of stress at constant length would follow rather a different curve if the actual loss of stress was large, but for the fairly moderate figures referred to in the Paper it should not be very different.

He had had the data for one of the curves in the Paper—the top curve in *Fig. 12*—re-plotted on a normal scale (*Fig. 19*), and it appeared to him that the upper points approximated to a straight line at least as well as they did to the Authors' logarithmic curve. If recourse was had to extrapolation, a striking result was obtained, as shown in *Fig. 20*. The

Authors' extrapolation led to the point A, the straight line led to B, and possibly one might select some intermediate point, C. In showing that curve, Mr Champion did not wish to suggest that the figures relating to B, or even to C, should be used for practical purposes; he merely wished to make the point that when the initial data were fairly scattered, and if

Fig. 19

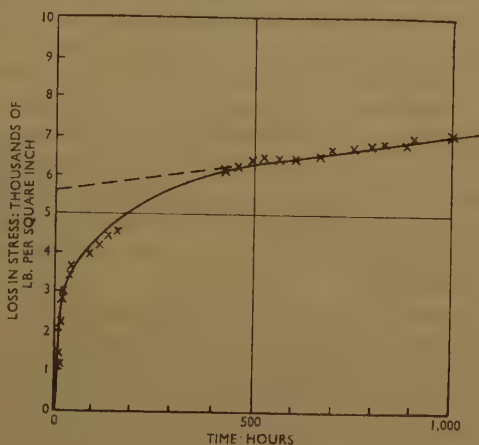
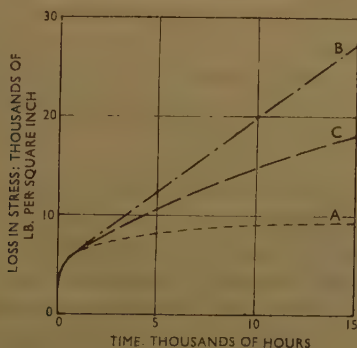


Fig. 20



extrapolation were then carried to extremes, the result was largely subjective and could not be relied upon to any great extent for practical work. He would suggest that the figures did not really provide any definite information about the ultimate creep to be expected after long periods. He noticed that the Authors themselves had said that longer tests were needed. He hoped that those tests had already been started and would be communicated to the Institution in due course.

Another interesting point was that the wires in most cases had not been initially straight but had had minute kinks in them. Each of those kinks formed, in effect, a curved tension member which was gradually straightened out by the effects of creep; but the straightening of one of those curved members would follow a different law from that of creep in the straight wire, so that the matter was further complicated by that effect. There was also the fact that, in practical cases, the wire might be held in the concrete before it was fully straightened, and on that account creep would be reduced.

Finally, he noticed that, although the Authors had given full details of the mechanical properties of the wires, they did not give any information on the composition or heat treatment of the steel. It was well known that in high-temperature creep those factors had an important effect on the value of the creep itself, and he would suggest that experiments to investigate their effect might be very useful. It was, in fact, possible that quite substantial differences in creep might arise from different wires that were commercially available.

Mr R. S. Brown observed that the properties of cold-drawn wire were so little understood, and so little had been published about them, that any Paper dealing with the properties of wire was of tremendous importance.

He had, however, failed to find in the Paper what he considered to be most important information on the history of the wires used for the tests. That was a matter to which Mr Champion had already referred. Mr Brown suggested that in the absence of knowledge of the history of those wires—how they had been produced, their composition, their method of manufacture and, more important still, how they had been handled and treated immediately after they were drawn—the tests described in the Paper were of limited value. He would like that to be borne in mind, and he referred to a Paper, on the properties of wires, which emphasized that point.⁶

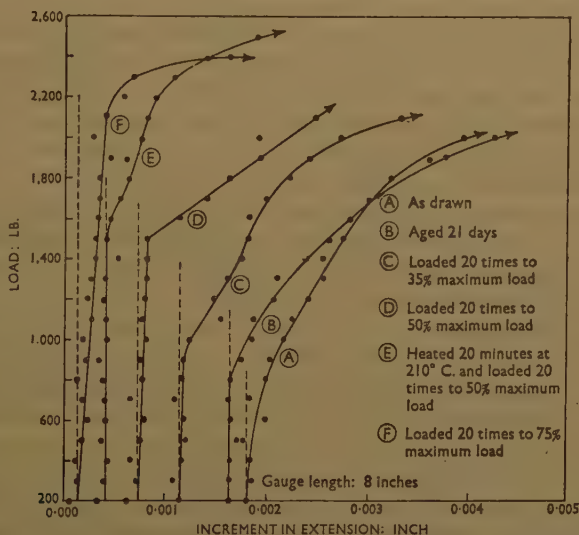
Mr Brown then displayed some slides, the first of which (*Fig. 21*) gave a series of load/strain increment curves, showing some very remarkable changes in the load/extension properties with various treatments after drawing the wire. Curve A was typical of the wire as drawn. It possessed no elastic properties and was just a continuous plastic curve. Curve B related to a piece of wire from the same coil, immediately adjacent to the first piece, aged 21 days; there was quite a substantial restoration of elastic properties. It would be appreciated that, since increments in extension had been plotted, a vertical plot was proof of elasticity in the wire. In the next curve (C), the wire had been loaded and unloaded to 35 per cent of its maximum load; in the next (D), to 50 per cent, and

⁶ R. S. Brown, "Plastic Strain and Hysteresis in Drawn Steel Wire." *J. Iron Steel Inst.*, vol. 162, p. 189 (June 1949).

in curve F to 75 per cent of the maximum load. Although there was not a complete restoration of the elastic properties there was very definite evidence of a substantial change from the first to the last curve by the simple effect of loading and unloading. The significant curve was E, for which the wire had been heated to 210° Centigrade; that curve exhibited the well known normalizing or strain-age-hardening effect which was noticed in severely drawn wires when they were heated at those low temperatures.

Mr Brown's second slide showed a graph of the stress/strain properties

Fig. 21



CHANGES IN DRAWN PATENT STEEL WIRE DUE TO AGEING, HEATING, AND REPEATED STRESSING

of cold-drawn wires under dynamic loadings, and the cyclic curves indicated the creep effect which took place at each loading. On the same graph were given a number of cyclic curves of wire whose tensile properties had been developed by heat-treatment, that was, hardening and tempering rather than by cold-working. There was a significant difference between the two sets of curves, the plastic features associated with cold-working being entirely absent in the heat-treated material.

Further examination had been made, loading and unloading the wires up to 100 times, and it could be seen from Mr Brown's third slide that the creep steadily diminished and that at the hundredth loading there was only a very small amount of creep; the curve had by then become almost completely cyclic, indicating that the wire had become stabilized. The

fourth slide showed a cold-drawn wire after heating at a low temperature; the initial plastic curve had disappeared and loading and unloading showed a more marked restoration of stability. Mr Brown pointed out that the curves he had shown were not creep curves but they supported his suggestion that unless the history of the wire, subsequent to drawing, was known it was not possible to deduce the true meaning of the tests recorded in the Paper.

A curious feature in the Paper was the 0.1-per-cent proof figures in Table 2. One fact which was known about wire was that the proof stress was increased by loading and unloading. The Authors' figures showed that most, if not all, of the 0.1-per-cent proof-stress values after test were lower, or tended to be lower, than the proof-stress values before testing. That was a surprising anomaly, and Mr Brown thought that some explanation from the Authors was called for. The other peculiarity was, he thought, that the wires which had been produced in 8-foot-diameter coils had obviously been subjected to very special treatment to exhibit the properties shown by the Authors. They had said, on p. 125, that "the creep loss in the steel has been considered in relation to the ratio of the applied stress to the ultimate stress, and also to the 0.1-per-cent proof stress, although the latter is a completely arbitrary quantity." Mr Brown would suggest that the 0.1-per-cent proof stress was a criterion, but it had to be used in conjunction with the known history of the material under test. In the present case, the 0.1-per-cent proof stress figures indicated, he thought, that the 8-foot-diameter coils had been subjected to some treatment other than just straightening. When a wire was straightened, the 0.1-per-cent proof stress was normally lowered, not increased, and therefore the inference drawn by the Authors from the 8-foot coils was wrong. Also, increased creep would be expected in wires straightened from coils. Would the Authors say something about the treatment and indicate whether he was right in his surmise concerning the latter point.

Had the Authors realized the implications that could be drawn from the results of tests comparing wire in 22-inch- and 8-foot-diameter coils? As the matter stood, one was led to conclude that the mere act of producing high-tensile wire in large-diameter coils would materially improve its creep properties. Whatever the merits of large-diameter coils might be, there was no reason why those should include a specific improvement in creep properties, unless at the same time some low-temperature tempering had been performed. Had the low-temperature tempering treatment been carried out also on the 22-inch-diameter coils, the creep results would have been very much in line with those of the 8-foot-diameter coils.

Dr K. Hajnal-Kónyi observed that he would like to make a suggestion relating to the title of the Paper. He noticed that both Authors and also the speakers in the discussion had used the terms "creep" and "relaxation" somewhat indiscriminately, whereas he thought that it was desirable to distinguish between those two terms and use "creep" for

change in strain under constant stress, and "relaxation" for change in stress under constant length. In his opinion, therefore, it would have been preferable to call the Paper "Relaxation of High-Tensile Steel Wire." In future Papers it would be desirable to make that distinction clear.

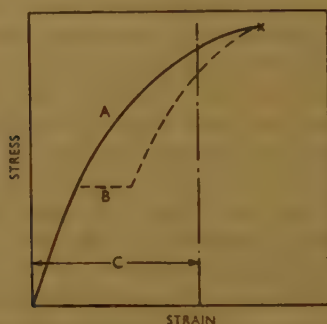
He would have liked to see the stress/strain diagram shown in *Fig. 7* continued. It was rather abrupt, and the shape of those curves was most characteristic if they were drawn up to their maximum stress. There was a great deal to be learned from that about the history and treatment of the wire. It could be seen even from *Fig. 7* that the 0.2-inch small coil and the large coil had quite a different stress/strain characteristic, which gave the inference suggested by Mr Brown. It would, therefore, be desirable if possible to include in the Paper a complete stress/strain diagram going up to the maximum stress. In future specifications Dr Hajnal-Kónyi would suggest the inclusion of a clause specifying the maximum uniform elongation which could be reached. That was quite easily determined on specimens by measuring the extension of the part where the specimen was not broken and no necking had occurred. That was a very important figure for the wire. The Paper would lead to the conclusion that the wire which was used in the large coils, and which was probably heat-treated, was very much to be preferred to the wires in small coils.

Professor A. L. L. Baker observed that the loss of prestress which was liable to occur in structures, corresponding to the results of the Authors' tests, would not be very serious, at any rate in structures in which slight cracking was not important; but in sleepers and in bridges, for instance, made of precast units joined together by prestressing, there would be an appreciable reduction in the factor of safety. That was because, in precast bridges, as soon as the pre-compression was overcome at the joints there was a tendency for one precast unit to slide over the other and for the resistance to shear to be at once almost zero, so that loss of prestress with creep in that type of structure would need to be watched very carefully. In sleepers, in which it was important to prevent cracking in order to prevent the concrete disintegrating with repeated loading and repeated changes of stress, and also in watertight structures such as water-towers, which were prestressed and in which there was at the beginning a factor of safety against cracking to produce watertightness, again the factor of safety would be slightly reduced by creep. He would say, however, that in floor beams in buildings, in which slight cracking did not matter very much, the effect of creep was almost immaterial.

He would like to ask the Authors if they had tested any of the wires to destruction afterwards, and if long-period creep had had any effect on the ultimate strain of the wires. He felt that that might be very important, because, with regard to the ultimate strength of beams, if the ultimate strain of the wires in spite of creep going on was about the same as without creep, then the ultimate strength of the member would be independent of the creep.

Professor Baker demonstrated that with the aid of *Fig. 22*. The typical stress/strain curve of steel wire would resemble A, without any creep occurring, but creep might start immediately after the prestressing operation, so that the stress/strain line would then run horizontally, as in B, until the member was tested to destruction, when the curve would

Fig. 22



rise in the way shown and probably finish up at the same ultimate strain. If C was the total prestrain in the member, then assuming that the distribution of strain just prior to failure in the member was linear, which it was, approximately, the balance would be the same whether the creep had taken place or not, so that the position of the neutral axis in the member would be the same. It seemed to him that if the ultimate strain in spite of creep taking place was the same, there would be no effect on the ultimate strength of the member.

Had the Authors noticed, in testing a number of wires, whether or not there was a wide variation in the value of the ultimate strain at failure? Most experimenters had found that there was not a great variation in the ultimate strength. Professor Baker believed that a figure was quoted in the Paper, but it was also important from the point of view of interpreting test results to know if there was a large variation in the value of ultimate strain or not. If there was a large variation, then in interpreting the test results it would be known that the strain at cracks in many tests would not be very different from the average strain taken from strain-gauge readings on either side of the crack. That was a point in which he was very interested.

* * Professor Robert de Strycker, of Louvain, welcomed the Paper as evidence of interest in Great Britain in a subject that had preoccupied Continental engineers for many years, and proffered some information based on experience gained at Louvain in the field of low-temperature creep.

* * This and the following contributions were submitted in writing upon the closure of the oral discussion.—SEC. I.C.E.

The Louvain station of the "Comité pour l'Etude du Fluage aux Températures Ordinaires" had been active for the past 4 years and about 400 creep tests had been made, the results of which had been published in but very incomplete form. An extensive report would be forthcoming in early 1953. That work was the continuation of a series of tests made for private companies, the Belgian Ministry of Works, and for research purposes at Louvain since 1943.

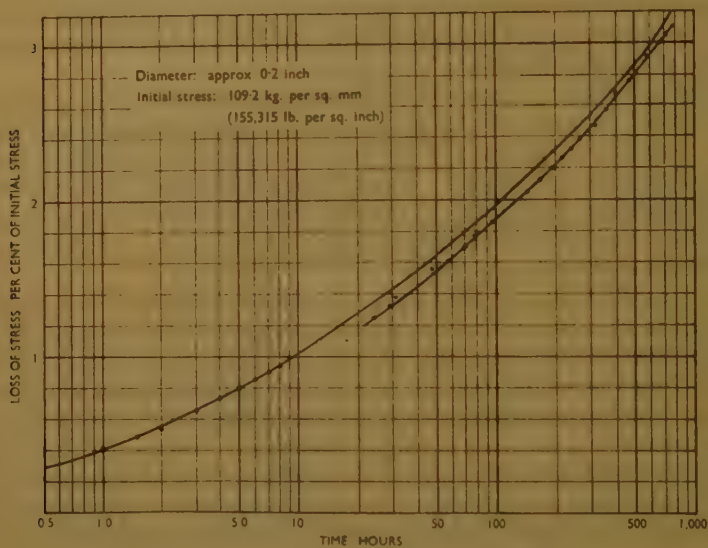
The importance of creep in prestressed concrete had been realized by Continental engineers several years before Professor Magnel's first Paper. Some tentative standards had already been adopted in Germany several years before its appearance. Many Papers other than those mentioned under references 2 to 5 (see p. 135) had been published, some of which included numerous experimental results. [A few of them are listed at the end of this contribution, p. 146.]

It was Professor de Strycker's experience that there was probably nothing to gain in accuracy by adopting long specimens: the greater the length, the greater were the chances of errors arising from thermal effects, from the weight of the measuring rig, and from the lack of rigidity of the rig. Non-homogeneous regions did occur in high-tensile wires but, for the more usual types, the scale of length necessary to average out those variations was of the order of many hundreds of feet, if not several thousands, whilst there were no significant differences between two specimens of few tenths of a foot apart. Great length of specimens unavoidably caused difficulties, on account of temperature variations. It was not clear from the Paper whether the water-tower in which the apparatus was set up was of concrete or steel; in either case, the vertical distance between the joists at the bottom and at the top was subject to variations that were neither equal to nor simultaneous with those of the tested wire, so that the stress in the wire varied not only because of the creep under "constant" length but also on account of the difference between expansion (or contraction) of the structure and of the wire. That probably accounted for part of the scatter of observed points around the curves of *Figs 9, 10, 11, and 12*.

From experience at Louvain, the rather abrupt variation of slope of the upper curve of *Fig. 12* was probably caused by slippage of the measuring rig. Such slippages occurred occasionally with all systems that were not actually soldered or welded to the wire. Soldering and welding were obviously unacceptable, therefore readings should be taken as often as possible in order to detect slippage as soon as it happened, and to compensate for it. After a week all rigs should be verified every working day; during the first week (and after the first day), several times a day. During the first night, the length was of course not constant, as could be readily observed on relaxation curves. *Figs 23 and 24*, representing some relaxation experiments made at Louvain, showed the influence of the lack of adjustment during the first night. The relaxation curves were shifted

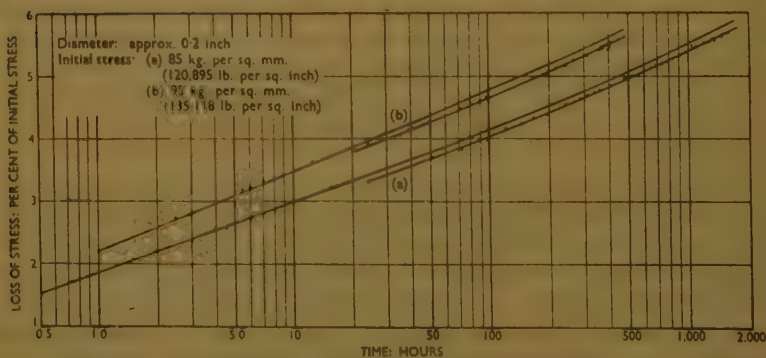
downward by approximately 0.08 per cent in *Fig. 23* and 0.12 per cent in *Fig. 24*.

Fig. 23



RELAXATION OF HEAT-TREATED STEEL WIRE

Fig. 24



RELAXATION OF HARD-DRAWN STEEL WIRE

The necessity of providing for independent anchorages was obvious; that also applied to the *upper* anchorages. German experimenters who had provided only for lower independent anchorages had found that many

disturbances disappeared when the upper anchorages were also made independent. That naturally precluded long specimens.

The method using a lighting contact set up as described in *Fig. 1 (a)* seemed to suffer from the drawback that the "pull-in" of the lower grip affected the total length. That error was probably relatively small in view of the large total length of the specimen. Experience at Louvain had shown that the effect of grips became small after about 15 minutes.

It had been found that for the usual design stresses the relaxation/log. time curves always had a positive second derivative, although the deviation from a straight line could be perceived only after several days for hard-drawn wires. Heat-treated and aged wires showed a curvature from the very beginning of the experiment.

It was not clear whether the Authors had made their experiments on coils of the same batch, some in the as-drawn state and others after straightening and re-winding on a large diameter. Professor de Strycker believed that that was not the case because the experiments at Louvain had constantly shown that the straightened and re-wound wire showed a *larger* creep. It was well known that straightening *reduced* the proof stress.

He did not believe any correlation should be sought between loss of stress and the ratio of initial stress to ultimate stress (*Fig. 14*). The upper curve was evidently ill defined, the lower one he considered to be fortuitous. The correlation with the ratio initial-stress/proof-stress was evidently more to be expected. Experience at Louvain, however, was that that correlation was not close enough to allow prediction of the departure of relaxation from proof-stress measurements, except in special cases.

Constant-length and constant-load experiments, although not strictly comparable, had nevertheless very high correlation.

Work done at Louvain had shown that there was some creep under all stresses for hard-drawn wire, even at $0.1\sigma_s$. For the same type of wire, the ratio of constant-load to constant-length creep was approximately $5/4$ during the first few days; it tended to increase slightly with time.

Dawance's method was very valuable because it allowed tests of very long duration to be made at low cost; the accuracy was, on the other hand, rather low and unpredictable. On the average the uncertainty in the stress was of the order of 0.5 per cent.

During most prestressing operations, the time during which the wire was subjected to stresses approaching the initial stress was of the order of a few minutes and the creep-loss after tensioning therefore remained very appreciable. On the average, the creep during the first 2 minutes was of the order of one-third of the subsequent creep in the next 3 days and of the order of one-fifth or one-sixth of that observed in the first 3-month period after the first 2 minutes.

It would be inadvisable to assume that the maximum loss of stress that would occur was that which was measured under constant length

during the first 10,000 hours. It was absolutely essential that more be known about the plastic shortening (creep) of the concrete and of the shortening due to drying and ageing. The concrete certainly did not shorten "elastically" as the Authors seemed to intimate; on the contrary, owing to the reduction of stress in the wires the concrete lengthened elastically. However, those elastic variations might be considered to be negligible.

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13. R. de Strycker, "*Le comportement sous tension des armatures pour béton précontraint*" ("The behaviour in tension of wires for prestressed concrete"). Congrès Inter. du béton précontraint, Communication B-42, p. 593 (Sept. 1951).
14. R. de Strycker, "*Fluage et relaxation des fils tréfilés*." ("Creep and relaxation of drawn wires.") *Rev. Métall.*, 48, p. 886 (1951).

Mr Pierre Kercavins and Mr Jean Simon of Paris, in a joint contribution, observed that they agreed with the Authors concerning the necessity of keeping a margin of safety between the initial force in the steel and its ultimate strength, for two reasons:—

- (a) To limit the risk of failure while tensioning; limiting the initial force to 75 per cent of the ultimate strength would seem to be too severe.
- (b) To limit the risk of failure of the steel in a beam when under load. The ratio of stress to be adopted depended not only on the stress in the steel, but also on the dimensions of the beam.¹⁵

The method of testing seemed to be very simple and ingenious, and would be useful for a number of different applications.

¹⁵ Robert Lévi, "*La sécurité du béton précontraint*" ("Safety in Prestressed Concrete"). *Travaux*, No. 215, p. 409 (Sept. 1952).

With regard to the wires tested, it was a pity that the tests had been carried out only on hard-drawn steel wire—that was to say, wires obtained by subjecting the same quality of wire to different degrees of drawing, which altered the state of internal stress—and that the Authors had not been able to use wires whose ultimate-strength value had been increased by heat treatment, and in which there was no internal stress.¹⁶

It was equally unfortunate that the Authors' tests had been limited to stresses equal to 80 per cent of the ultimate strength. Tests carried out at stresses largely in excess of the 0.1-per-cent proof stress might have revealed great differences between the drawn wire and the wire obtained using heat treatment.

The creep curves given in the Paper were strictly comparable to all others which Messrs Xercavins and Simon had seen. They shared the Authors' opinion that only a diagram with a semi-logarithmic scale allowed a true estimate to be made of the degree of relaxation.

Figs 13 to 17 and the formulae mentioned in the Paper all related the relaxation to different stress characteristics of the steel such as 0.1-per-cent proof stress, AFNOR elastic limit, or ultimate strength. The scatter of the results obtained did not permit a choice to be made between the various relationships suggested at the present time. The following considerations suggested a need to look in other directions for the factors which influenced the relaxation.

All the formulae given in the Paper indicated that steels were required that gave stress/strain diagrams with the longest possible straight initial portion, which could be obtained by cold-working when drawing. It had been found in a great number of cases that wires with such characteristics did in fact give small relaxations; nevertheless, for steels which were severely drawn it had been found at the same time that although those characteristics and relaxations remained small in the first few hours, they greatly increased in the course of time. A typical case was that of wires drawn down to 2.5 or even 5 millimetres diameter, in which the drawing had been sufficient to reduce considerably the elongation at failure. The Authors' own results, moreover, bore out that statement (for example, in the case of the 12 S.W.G. wires).

It was stated on p. 119 that, for a given quality of wire, the relaxation could perhaps be related to the initial stress, but if an attempt was made to compare the results obtained for wires of different qualities, it was revealed necessary to introduce "some property" of the wire. It seemed that that property bore some relation to the internal stress in the wire, and that could best be represented by the elongation at failure. For that reason, Messrs Xercavins and Simon had proposed at Ghent the

¹⁶ Louis Laravoire, "*Un nouveau produit sidérurgique français—le 'fil machine' en acier traité pour béton précontraint*" ("A New French Siderurgical Product—Drawn Steel Wire treated for Prestressing"). *Travaux*, No. 217, p. 523 (Nov. 1952).

formula¹⁷ which the Authors had presumably read; that formula had been selected in preference to others (some of which were similar to those mentioned in the Paper) and favoured the use of wire whose ultimate strength was increased by heat treatment—that was to say without reducing the elongation at failure—and not by cold-working. The Authors' attention was again drawn to the Paper by Mr Laravoire, referred to previously.

The Authors had found a much larger relaxation for the wire in small-diameter than in large-diameter coils. Messrs Xercavins and Simon had obtained the same result, with the reservation that it applied only to wire wound directly to that diameter, and not to the large coils obtained by straightening the wire originally wound on small coils, which gave, on the contrary, much larger creep.

In their reply, could the Authors show the values for elongation at failure obtained in their tests?

Messrs Xercavins and Simon were trying to obtain precise data on that property, and for that reason were measuring in every test the elongation at failure, with necking and without necking, on lengths of 33·3, 100, and 200 millimetres.

Mr C. F. Brereton observed that Mr R. S. Brown had suggested that the prior history of wires used should be stated in order to make the creep figures of greater value to designers. In the case of the coils of 0·2-inch wire, in both large- and small-diameter coils, and the 0·276-inch in 8-foot diameter coils, all were from basic open-hearth steel, the additives being 0·8 per cent carbon and 0·75 per cent manganese, which had been rolled to rod in a continuous mill. The rods were patented (lead cooled) and, after descaling, cold-drawn to size. The "small" coils, after aqueous alkaline desoaping, washing, and drying, were delivered in the coil form stripped from the wiredrawing block. In the case of the 8-foot coils, further processes were carried out to remove the curvature imparted on the wiredrawing block, and to give a stabilizing treatment designed to reduce creep or relaxation in the finished wire. The large diameter of 8 feet had been selected to cover the 0·276-inch wire so as to overcome any permanent-set effects on re-coiling the straight wire. The same-diameter block was used for the 0·200-inch wire solely because users found it inconvenient to have more than one size of pay-off reel, and it had found favour among many users who now relied on the wire paying-out quite straight without having to use the "on site" straightening machinery, which had previously been used on the small coils, and had also resulted inevitably in an alteration in wire properties in the form of a lowered 0·1-per-cent proof-stress value. It was believed that the "straight"

¹⁷ J. Simon and P. Xercavins, "*Le caractère conventionnel de la limite d'élasticité dans les aciers durs de précontrainte*" ("The Conventional Nature of Elastic Limit in H.D.S. Wires in Prestressed Concrete"). Congrès International du Béton Précontraint, Ghent, 1951. Report, p. 431. (Pubn No. B.28.)

large-diameter coiled wire had adequate properties at the usual design stresses currently in use, and the lower relaxation values reported by the Authors indicated that it was a considerable improvement on the previous small-diameter coil. In producing those special wires for prestressed concrete work, it was considered desirable to retain all the known properties of cold-drawn wire, particularly in relation to possible corrosion fatigue, and to supplement those with enhanced elastic properties by the well tried method of controlled low-temperature heat-treatment applied in such a way as to give uniformity.

Mr Walley, replying on behalf of Mr Clarke and himself, thanked the speakers for the kind way in which they had received the Paper. The Authors had realized, when presenting it, that they would meet some criticism of various points. He had tried in his opening remarks to the meeting to forestall some of it, but it was obvious that he had not quite succeeded. He would like to reiterate what had been said when presenting the Paper, namely that the Authors had tried to keep the work related to the behaviour under load of commercial wires which they had received in the ordinary way from suppliers for use in prestressed concrete; the Paper did not attempt to deal with the various factors which influenced the relaxation of the wires.

On Mr Champion's first point, about the effect of high stresses causing creep, Mr Walley would rather say that it was the nature of the wire and not the stresses which had raised that important question at the present time. A few remarks about that appeared on p. 119. He thought it would be found that such creep or relaxation took place at relatively low stresses. It had been observed on the Continent at stresses of the order of one-tenth of the ultimate tensile strength, so that the phenomenon could take place at almost any stress, and its cause was really the quality of the wire and not the stress.

Mr Champion had asked whether the Authors were right to extrapolate to 10,000 hours or even longer. That, Mr Walley agreed, was bound to be open to criticism until tests had been carried on for a long time. At the present time the work which was being done at the Building Research Station still seemed to indicate that it was true for a much longer time than 1,000 hours, at any rate for the small-diameter-coil wire; there was some uncertainty about whether it was true for the large-diameter coils. The longest test which he had ever heard of was one not done in connexion with prestressing operations, but in connexion with tests in the United States on high-tensile steel wire as used in suspension-bridge cables. They had gone on for $14\frac{1}{2}$ years. That was not the expected life of a building, but it was a long time, and certainly there had been a decrease in the rate of creep or relaxation during that time, and a stable condition had been reached, even under excessively high stress. He did not think that there was any evidence that the rate of relaxation would increase after a period of time, and for that reason he did not think that

curves B and C were at all realistic, but he would be glad to see the references mentioned. The general creep characteristics of all materials seemed to follow a logarithmic law for loss. He was sorry that Mr Champion thought it deceitful to use semi-log paper, but Professor de Strycker had done so in his written contribution so they were in distinguished company.

With regard to Mr Champion's point about the wire on the small coils having kinks, there were in fact no kinks in that wire, or at any rate anything that Mr Walley would describe as kinks. It had, of course, to be straightened under the load, but the wire was straight, so far as he could see, with that load on it. The effect of that, as opposed to wires stressed to run out straight was one of the more important points brought to light in the Paper. Whatever was the reason for it, there was that difference, which had been found between wires delivered in small and in large coils.

He agreed with Mr Champion that different amounts of creep would be obtained for different wires, and that was partly why the Authors had started the programme of work in the first place. They had the results of Professor Magnel's work in Belgium, but they did not think that those results were necessarily correct for the wire which the British engineer usually obtained, and so they had started on the work described in the Paper. There was obviously very different creep for different wires, particularly if they were made by different methods.

Both Mr Champion and Mr Brown had raised an important point in asking for the history of the wires. It was hoped that the manufacturers of the wires which the Authors had used would agree to supply that information for publication later. It was, of course, important to give a complete picture, particularly from the point of view of research.

Mr Brereton had kindly supplied the information on the treatment to which the wires in the large coils had been subjected and indicated that the wire on the large coils was heat-treated. Whether it was so important from the point of view of the practising engineer Mr Walley was not so sure, because the average practising engineer did not ask for that; he asked for his wire to fulfil certain tests which he laid down and was not really interested in its history. The results which had been given purported to show the effect of creep on wire which the engineer was at present getting. Without the data given, Mr Walley did not know what value the engineer took for his losses. It was necessary to have some figures for them or to assume some completely arbitrary value for the loss due to creep or relaxation. The figures which the Authors had given would provide some idea of the losses to assume.

Mr Brown could not understand why the 0.1-per-cent proof stress was sometimes less after the test than before. Mr Walley had glanced through the figures in Table 2, and it was not invariably less after the test than before; there seemed to be equal numbers lower and higher. For that reason the Authors had drawn the conclusion that it did not seem to make

much difference to the shape of the stress/strain curve. Mr Walley could not offer any explanation of that. Mr Brown's surmise about the heat-treatment of the wire had been correct. The wire was not simply straightened and run-off on to large coils.

The point which Mr Brown had made about getting a lower proof stress and a higher creep on simple straightened wire was one which had been found by certain investigators on the Continent, and both Professor de Strycker and Mr Simon had drawn attention to it. He was glad to be able to confirm that the present results were not contrary to that since it was now known that the straightened wire had been heat-treated.

Mr Brown's last point was an extremely important one. The Authors would not like it to be thought that simple straightening of the wire would improve the creep characteristics. They realized that the statement at the top of p. 126 might seem to imply that, but it really referred to the types of wire commercially available in England in 1952. They would agree that the heat-treatment which the wire on the large coils had been given was the prime factor in reducing the creep, but could not confirm or deny that the creep results on the 22-inch-diameter coils, similarly heat-treated, would be similar. The cold-work necessary to straighten the wire for use in construction work would no doubt again affect the result.

Dr Hajnal-Kónyi had taken the Authors to task for being loose in the use of the words "creep" and "relaxation." Mr Walley thought that it was only in the introduction and the title that they had used the word "creep." They had used the word "relaxation" in all material places, but they had hesitated to use it in the title, because they were afraid that people would not understand what was meant, but would have heard of the word "creep" in relation to concrete and steel. They had been afraid of frightening people off by using "relaxation." It was agreed that "relaxation" should be used for tests at a constant length and "creep" for tests at constant stress.

Dr Hajnal-Kónyi had asked whether the stress/strain curves in *Fig. 7* could be enlarged to take account of the strain up to failure. All those stress/strain curves had been obtained for the Authors by a testing house; the Authors had not tested the wires, because they had no apparatus to do so. The testing house had a marked disinclination to test wires to failure, because of possible damage to their instruments. Mr Walley would like to see the curves taken further, but it might be a very expensive business, if the instruments were damaged.

Professor Baker had raised the question of factors of safety and the effect of creep or relaxation on them. Mr Walley agreed with his remarks concerning the factor of safety against cracking, but could not see how it would affect the ultimate conditions, even if the unit was made up of small precast members joined together with prestressing cables or wires, unless premature failure occurred through shear. That the effect of

creep of steel and concrete on the ultimate strength of a prestressed member might be small was demonstrated by some tests¹⁸ carried out on long-term loading of prestressed concrete joists, even after loading to 90 per cent of the ultimate for many months.

On removing the load and then re-testing to failure, no significant difference could be detected on the ultimate carrying capacity of the beam. The strains which had taken place in the wire at failure, were shown in Table 8. It would be seen that there was very little change in the ultimate

TABLE 8.—STRAIN AT ULTIMATE STRESS OF HIGH-TENSILE STEEL WIRE
(GAUGE LENGTH : 2 INCHES)

(To be read in conjunction with Table 2)

Test No.	Before test	After test
1	9.1	7.6
2	9.1	6.7
3	9.1	6.7
4	9.1	4.7
5	4	3
6	4.7	4
7	4.7	4
8	4	5.7
9	4	5.7
10	4	3
11	7	6
12	7	7
13	8	7
14	7	7
15	7	7
16		10
17	7	6
18	6	6
19	7	8
20	6	6
21	6	6
22	5.5	5.5
23	6.5	6.5

strain as between before and after test. The Author had no information on the strain of the wire at failure in a prestressed beam so could not say whether or not it was different from the strain obtained in a simple tensile test.

The Authors thanked Professor de Strycker for his informative criticism of the Paper, and looked forward to seeing his full report of the Belgian

¹⁸ F. Walley, "Holding Tests on Pre-tensioned Prestressed Concrete Joists." *Congress International du Beton Précontraint*, Ghent, 1951, Paper No. B. 30, p. 461.

tests in due course. They were aware of that work, but since there was so little of it published they could only note it in passing. They were not aware of any early tentative standards in Germany, and German Code, 4th Draft (1945) for prestressed concrete made no mention of the subject, but they were grateful for the additional references given. Although they were aware of the majority of them, they were not included in the bibliography on p. 135 since the Paper had been drafted before they became available. The greater part of the work which they described, however, fell under the general criticism of other Continental work in so far as it described tests of "creep" and not "relaxation," and had been carried out for only very short durations.

The Authors, whilst hesitating to disagree with Professor de Strycker, suggested that greater accuracy was obtained using a long length when carrying out the experiment in the manner described, although they agreed that that was not necessarily so with other means of measurement. Present work at the Building Research Station used short specimens. Variations caused by thermal movements within the building were taken care of, except for the first hour, by using reference wires, and thereby a length datum which was independent of the building, as described in the Paper, so that could not be the explanation of the scatter of the observed points: the Authors considered that the more probable cause of that scatter was the relative precision of the large-scale experimental technique as compared with the micro-measurements obtainable in a laboratory.

Figs 23 and 24 were interesting from two points of view: first, the differences caused by not adjusting for the creep at constant load during the first night (it was for that reason that it was considered that the relaxation was too high in the Authors' experiments and therefore conservative), secondly, the difference in relaxation shown between the heat-treated and the hard-drawn steel wire; the values given from those figures for the relaxation at 1,000 hours, assuming the ultimate strength of the wire to be 105 tons per square inch, corresponded closely with *Fig. 14* (p. 126)—the heat-treated wire of *Fig. 23* with the left-hand curve which was now known to have been heat-treated, and the hard-drawn wire of *Fig. 24* with the right-hand curve.

As explained earlier in the reply to Mr Brown, Professor de Strycker was correct in assuming that the wire was not the same in the small and large coils.

The correlation between loss of stress and the ratio of initial to ultimate stress was one which the Authors had suggested as a practical method of illustrating the data. It was not one which they particularly liked, but one which had been forced upon them since other means of correlating the data seemed to give a greater scatter. Mr Simon has sought another means of plotting the results. The method adopted in the Paper had the merit, though not a scientific one, of using a relationship based on data which were available to the designer of prestressed concrete. Professor de

Strycker's figures for the relative amount of creep in different times were valuable, and perhaps illustrated the point that experimental data were required on the relaxation which took place in practice and not in the laboratory.

The figure of 10,000 hours seemed to the Authors to be a reasonable compromise for the limit of relaxation, and they were glad to see that in a Paper by Mr Simon the same figures had been taken for practical purposes.

Professor de Strycker was, of course, correct in saying that the concrete extended elastically because of the plastic shortening and the relaxation of steel. The initial elastic shortening did, however, reduce the relaxation.

In reply to Messrs Xercavins and Simon the Authors wished to say that the tests had been carried out on wire which was commonly used in the prestressing industry in Great Britain. At the present time, steels in which high ultimate strength was obtained by heat-treatment were not available in large quantities in Great Britain. The tests had been limited to stresses of 80 per cent of the ultimate strength because the Authors felt that that was the useful upper limit to which hard-drawn wires could be safely stressed in practice.

The Authors were aware of the formula proposed by Messrs Xercavins and Simon for relating relaxation to the ratio of the initial strain to the strain at failure, and had plotted their results on that basis, but with no success. The elongation at failure was given in Table 8.

Messrs Xercavins and Simon were of the opinion that the heat-treated steels would prove superior to the hard-drawn steel, but the Authors felt that it was too early to draw such a conclusion and thought that the question of corrosion-fatigue in any oil-quenched wire was a subject which needed to be cleared up before such steel was used for civil engineering work. It was understood that several service failures had occurred in the United States because of corrosion-fatigue in steel-wound pipes.

The Authors were pleased to receive Mr Brereton's contribution and so to be able to clear up the doubts surrounding the manufacture of the 8-foot coils. It was hoped that his contribution would satisfy all those who had asked for that information.

Finally, they would like to draw attention to two other Papers which had a bearing on the subject. The first,¹⁹ by Dawance, described tests on the relaxation of steel and creep of concrete, and the second²⁰ reviewed the properties of prestressing steel.

Correspondence on the foregoing Paper is closed and no contributions, other than those already received at the Institution, can now be accepted.—SEC. I.C.E.

¹⁹ G. Dawance, "*Expériences de relaxation des contraintes dans le béton précontraint*" ("Tests concerning creep and shrinkage losses in prestressed concrete"). Int. Assoc. Bridge & Struct. Engng, vol. 12, 1952, p. 109.

²⁰ Fritz Schwier, "*Stahldrahte für Spannbeton*" ("Steel wire for prestressed concrete"). Beton. Stahlbetonbau, vol. 47, p. 201 (Sept. 1952).

ELECTION OF MEMBERS AND ASSOCIATE MEMBERS

The Council at their meetings on the 18th November and the 16th December, 1952, in accordance with By-law 14, declared that the under-mentioned had been duly elected as :—

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WILLIAM HENRY GORDON ROACH, M.B.E.

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DEATHS

It is with deep regret that intimation of the following deaths has been received.

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- JOHN BRAGA CALLANDER (E. 1922, T. 1930).
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 VIVIAN BOLTON DOUGLAS COOPER (E. 1886, T. 1914).
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 SYDNEY BRYAN DONKIN, (Past-President) (E. 1898, T. 1912).
 ALFRED CHARLES GARDNER, F.R.S.E. (Member of Council) (E. 1909, T. 1918).
 JOSEPH WILLIAM HUSBAND, B.Eng. (E. 1924, T. 1939).
 Professor ALEXANDER HOPE JAMESON, M.Sc. (E. 1901, T. 1912).
 JOHN SPENCER KILICK, C.B.E. (E. 1908, T. 1917).
 EDWARD HAROLD MORRIS (E. 1900, T. 1928).
 FREEMAN GERALD SKETCH (E. 1919).
 Lt.-Col. ALEXANDER SLATER, M.B.E. (E. 1913, T. 1944).
 ROBERT STIRLING (E. 1898).
 THOMAS WILLIAM TOWNSEND TUKEY, B.E. (E. 1890, T. 1900).

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Student

- JOHN IRVINE HUMPHREY (A. 1947).

Paper No. 5883

**“The Distribution of Sulphates in Clay Soils and
Groundwater” ***

by

**George Edward Bessey, M.Sc., and Frederick Measham
Lea, C.B.E., D.Sc.**

(Ordered by the Council to be published with written discussion) †

SYNOPSIS

Deterioration of concrete in clay soils containing sulphates has been experienced in various parts of Great Britain. The present Paper records the results of a detailed examination of the sulphate content of sites on the Keuper Marl, Lias, Oxford, Kimmeridge, and London clays, and one on an estuarine alluvial marsh clay. The wide variation of sulphate content of the clays with depth and from point to point, the seasonal variation in the sulphate content of the groundwaters, and the effect of drainage in transferring sulphate salts from one area to another are demonstrated. The application of the results to the examination of sites and to the assessment of the potential risk of sulphate attack is discussed, and the precautionary measures desirable to protect concrete from deterioration are outlined.

INTRODUCTION

INVESTIGATIONS into the corrosion of cement products, buried or partly buried in the ground, were under consideration before World War II by a Sub-Committee of the Research Committee of the Institution of Civil Engineers. Arrangements were being made for a large-scale investigation into the deterioration of concrete, particularly in sulphate-bearing soils, and a survey of sites which might be suitable for field tests was begun in 1937. This work was interrupted by the outbreak of war, and it has not so far been possible to continue it. Some account of the survey work done in 1937 and 1938 may be useful, since some interesting results were obtained demonstrating the variations in sulphate content of soils from point to point in a restricted area, variations with depth at any one point, and the variations in the groundwater with the season. Whilst it is known that occurrences of sulphates are sporadic, and that local variations may occur,

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† Correspondence on this Paper should be received at the Institution by the 1st July, 1953, and will be published in Part I of the Proceedings. Contributions should be limited to about 1,200 words.—SEC. I.C.E.

no detailed results of any examination of sites in Great Britain have previously been published to show how considerable these variations may be, and how misleading, therefore, may be the analysis of single samples.

The sulphates of calcium, magnesium, and the alkali metals occur widely in the Mesozoic and Tertiary clays of Great Britain, including the Keuper Marl, and Lias, Oxford, Kimmeridge, Weald, Gault, and London Clays. A general indication of the distribution of these clays, and of the areas in which they are not covered by superficial deposits, was given by Dines.¹ The superficial deposits or drift are, so far as is known, substantially free from sulphates, and it is only those areas where the sulphate-bearing clay beds come to the surface, or are covered by a very small thickness of drift, which are of practical importance. Numerous cases of deterioration of concrete have been reported in many parts of the London Clay and in other sulphate-bearing clays.

The protective measures and the types of concrete most suitable for use in any particular conditions of exposure to sulphate groundwaters have been discussed in earlier Papers,^{2, 3} and are summarized in Appendix II. The data given in the Paper will, it is hoped, assist engineers in assessing the severity of exposure and in deciding on the appropriate steps to be taken.

SITES EXAMINED

The sites which were examined included Keuper Marl, Lias, Oxford, Kimmeridge, and London clays, and also one of estuarine alluvial marsh clay; they are listed in Table I. The National Grid map location of most of them is given in the Table, so that the positions may be found, if required, on Ordnance Survey maps. Of these sites, A, B, C, E, I, K, and P were examined in some detail and several borings taken on each, whilst samples from only one boring or pit were taken on sites D, L, Q, T, U, W, and X. Some of the sites (B, T, U, W, and X) were found, in the preliminary tests, to have soil and groundwater of low sulphate content and only two of these (B and T) are described. The results on one site (K) were referred to in a previous Paper,² but are given here in more complete form. Sites P and L were used in 1944 for field tests on the corrosion of metal pipes, carried out by Dr J. C. Hudson for the Corrosion Committee of the Iron and Steel Institute (now of the British Iron and Steel Research Association).

Procedure

After preliminary examination of the site to ascertain its suitability for field exposure tests, in respect of slope, drainage, etc., the site was surveyed for levels and, where no suitable reference point was available for use as a bench mark, a concrete block was cast in the ground. Suitable

¹ The references are given on p. 177.

TABLE I.—LOCATION OF SITES

Location of site	Map reference (National Grid)	Site identification mark	Geological age of subsoil	Surface character of site
Gotham, near Nottingham	4540.3300	P	Keuper Marl (probably re-deposited as alluvium)	Flat; poor drainage
Cheltenham Sewage Works	3908.2231	E (also F and G)	Lower Lias	Slightly sloping (average 1 in 25)
Glastonbury	—	X	—	—
Chippenham	—	T	Oxford Clay	—
Yeovil, Wiltshire	—	W	—	—
Shepton Mallet, Somerset	—	U	Middle Lias	Moderate slope; well drained
Gillingham, Dorset	—	Q	Kimmeridge Clay	—
Ilford (Clay Hall), Essex	5441.1903	A	London Clay	Slightly sloping; well drained
Ilford (Hainault Recreation Ground), Essex	5459.1915	B	" "	Flat; well drained
Romford (Bedfords Park), Essex	5513.1919	C	" "	Steeply sloping
Romford (Heaton Grange)	5525.1917	D	" "	—
Winkfield, Bracknell, Berkshire	—	I	" "	Flat
Benfleet (Thundersley Glen), Essex	5789.1868	K	" "	Steeply sloping
Benfleet (pumping station), Essex	5775.1862	L	Alluvium	Flat marsh

points were then chosen for boring, to give an indication of the variations of soil and groundwater in the area which might be used for exposure tests. Borings were in most instances carried out with a 5-inch post-hole auger, fitted with extension handles to give bores 10 feet deep, though occasionally trial pits were dug. The soil removed by the auger was collected and sampled to give average samples of each 2 to 3 feet of depth, or, where the character of the soil changed, of each band of soil through which the boring passed. Samples weighing several pounds were taken on the site and were dried, mixed, and reduced in the laboratory for analysis. Total sulphate and water-soluble sulphate, lime, and magnesia contents were determined on many of these samples.

The boreholes were fitted with sheet-zinc liners to a depth of 2 feet, and zinc caps, made to fit, were placed in these liners to exclude rain and surface water.

Groundwater levels were taken immediately after boring and, on some sites, again at later dates; groundwater samples for analysis were collected at the time of boring and also at later dates. These water samples were

examined for *pH* value, and for sulphate, chloride, lime, and magnesia contents.

Site P.—Gotham, Nottinghamshire (Keuper Marl)

The only site nominally on the Keuper Marl was actually on an alluvial flat in the Trent Valley and the subsoil to the depth examined was alluvium, consisting essentially of redeposited Keuper Marl. It was on enclosed land adjacent to sewage treatment plant of Basford Rural District Council. Two pits were dug, about 140 feet apart, to a depth of 6 feet. The soil throughout consisted of dark brown clay with many large crystals, or masses of crystals, of gypsum (calcium sulphate). There were also thin beds, or lenticles, of peaty matter. The groundwater level at the time of excavation (November 1938) was about 18 inches below the surface, and the ground was said to be waterlogged in wet periods.

The analyses, given in Tables 6 and 7, show the proportion of calcium sulphate to be very high indeed on this site, and the groundwater to be saturated solutions of calcium sulphate with some additional sulphate, possibly as alkali sulphate (the solubility at 25° C. of calcium sulphate in pure water is equivalent to approximately 125 parts of SO_3 (sulphuric anhydride) per 100,000). There is very little chloride or magnesia.

The site is an extreme case and should not be regarded as typical of the Keuper Marl, although large amounts of calcium sulphate are found in some other places, and examples of attack on concrete by sulphate-bearing waters in this formation are known.⁴

Site E.—Cheltenham, Gloucestershire (Lower Lias)

The site at Hayden, near Cheltenham, on Lower Lias Clay, adjoined the sewage works of the Borough of Cheltenham. Seven boreholes were examined, distributed over an area about 100 feet by 75 feet.

The subsoil consisted throughout of clay, mainly blue or grey, but with some brown in colour. Traces of fine gypsum crystals were visible in samples from several of the bores at depths below 3 feet, but they were confined to particular thin layers. Analyses of several samples of soil from three of the boreholes at depths down to 5 feet showed less than 0.1 per cent acid-soluble SO_3 , and it was concluded that there was very little sulphate in the soil itself. The analyses of groundwater given in Table 7 show, however, very considerable amounts of sulphate, some being magnesium and some sodium sulphate; the two analyses differ considerably and it is obvious that the salts in these groundwaters are derived from soil elsewhere, and that the concentrations depend upon the flow in the more porous parts of the ground. Any attempt to assess the risk of sulphate action on concrete in this site from analyses of the soil alone would be completely misleading.

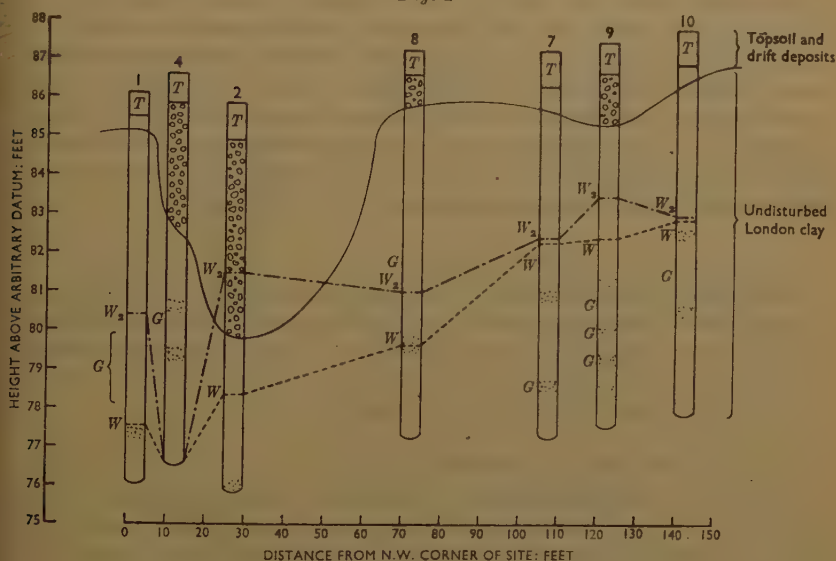
Confirmation that the sulphates were derived from neighbouring ground was obtained from two other boreholes on slightly higher ground, one

(No. F1) about 250 feet, and the other (No. G1) about 1,000 feet from the site; these showed many gypsum crystals at depths below 3 feet, and very high sulphate and magnesia contents in the groundwaters (see Table 7). The ratio of acids to bases determined in these waters also indicated that a high proportion of the sulphate was present as the sodium salt. Severe deterioration of buried concrete through sulphate action had been observed in this region.⁵

Site A.—Ilford, Essex (London Clay)

A detailed survey was made of a site adjoining a proposed school site at Clay Hall Road, Ilford, Essex. The area examined was about 200 feet

Fig. 1



LEGEND

- Stony or calcareous clay
- Sandy layers
- G* Appreciable visible gypsum
- W* Water level at 7. 10. 37
- W_a* Water level at 12. 11. 37

BOREHOLES AT SITE A, ILFORD, ESSEX

in length from North to South, and varied from 100 to 200 feet in width from East to West. The slope from S.E. to N.W. was nowhere more than 1 in 60. Seven boreholes were made, each to a depth of 10 feet. Fig. 1 shows the surface levels, groundwater levels (a few days after boring, and

about one month later—on 17 October, 1937), and also the beds passed through during boring. There was a deep pocket or channel of drift deposit around boreholes Nos 2 and 4 but otherwise there was undisturbed London Clay below about 2 feet. Thin sandy or silty layers occurred somewhat irregularly and gypsum crystals were visible in places in most of the holes below about 6 feet. The groundwater levels were not related to the surface levels and appeared to depend upon the presence of the sandy layers, the clay being relatively impermeable to water. The borings were carried out at the end of the dry season; groundwater levels then were low but a month later they were considerably higher, particularly in borehole No. 2.

The analyses of selected samples of groundwater are shown in Table 7. The samples are of fairly uniform sulphate content, even though taken on different dates, except for No. 2 borehole which is much lower than the others. There is little doubt that this was because the surface drainage came through the gravelly material at this point. The sulphates were present as the calcium, magnesium, and sodium salts, and there was an appreciable amount of chloride, varying from one bore to another. No analyses of the soil were made, but it was apparent from inspection that the calcium sulphate present varied very widely with depth.

Site B.—Ilford, Essex (London Clay)

Three bores were put down in the N.E. corner of Hainault Recreation Ground, in the northern part of Ilford. In all three holes the soil to a depth of several feet consisted of mixed glacial drift. There was no groundwater down to a depth of 10 feet, and none of the samples of soil was found to contain more than 0.05 per cent of sulphate.

Site C.—Romford, Essex (London Clay)

The site in Lower Bedfords Park, Romford, measured approximately 100 feet by 140 feet, with a slope of about 1 in 10 across the site, and 1 in 35 along its length. Six boreholes were made, showing drift consisting of calcareous clay, clay with flint pebbles or mixed silt and sand, to a depth of several feet. At greater depths, particularly in the lower part of the site, blue or brown clay was found with some visible gypsum. The clay was probably redeposited and not undisturbed London Clay. All the boreholes were dry at a depth of 10 feet when bored in October, but some showed water at depths of 5 to 6 feet within a month, and to within a few inches of the surface in March of the following year.

A number of samples were analysed with results shown in Tables 6 and 7. The soil samples, with the exception of one from a depth greater than 6 feet, were low in sulphate. No water samples could be taken the day the holes were bored, since all were dry, and only one contained any water the following day. This sample and others taken a month later all showed high sulphate contents, with much sodium and magnesium sul-

phate. The sulphate in the groundwater is, no doubt, derived from soils in an adjacent area from which the water drains.

This site provides another example of the erroneous conclusions which might be drawn by sampling only the soils on a site, even though several samples were taken. It would only be possible, from the soil analyses, to conclude that the ground was free from serious amounts of sulphate and that concrete could be buried to depths of 6 feet (or from the results of some boreholes, even deeper) without risk of sulphate action; the groundwater concentrations, however, and the high level of the groundwaters in the wet season, indicate that there would be a serious risk of sulphate attack on ordinary Portland-cement concrete.

Site D.—Romford, Essex (London Clay)

A single borehole was made in ground adjacent to the Heaton Grange Reservoir of the South Essex Waterworks Co., near Romford. High sulphate concentrations in the groundwater on this site had previously been found and precautions had been taken to avoid sulphate attack on concrete work.⁶ Unprotected concrete of 1 : 1½ : 3 proportions was observed to have suffered attack to a depth of about 1 inch in a period of 3 to 4 years. Lean concrete (1 : 2·7 : 5·4) showed heavy attack within 9 months of laying.

The borehole passed through wet London Clay with a bed of porous sandy clay at a depth of about 5 feet. Although tiny crystals of gypsum were found, the sulphate contents of two samples analysed (Table 6) were small.

TABLE 2.—WATER LEVELS IN BOREHOLES, SITE K, BENFLEET

Borehole No.	Water level (depth from surface)	
	December	March
1	8 ft. 2 in.	1 ft. 1 in.
2	7 ft. 0 in.	1 ft. 7 in.
3	7 ft. 5 in.	2 ft. 1 in.
4	Dry	0 ft. 7 in.
5	7 ft. 5 in.	1 ft. 0 in.
6	5 ft. 10 in.	1 ft. 4 in.
7	9 ft. 9 in.	1 ft. 3 in.
8	Dry	0 in.

The groundwater level rose to a depth of 5 feet from the surface soon after boring, and the analysis of a sample of this water showed it to contain a high proportion of calcium and magnesium sulphates. On this site also, therefore, the analysis of the soil samples alone would give a misleading result.

TABLE 3.—ANALYSIS OF GROUNDWATERS, SITE K, BENFLEET

Borehole No.	Date of Sampling	pH value	Analysis (parts per 100,000)			
			Sulphate (SO ₄)	Chloride (Cl)	Lime (CaO)	Magnesia (MgO)
1	December	8.5	218	29	68	68
2	December	7.5	278	76	76	94
	March	8.3	175	110	54	75
3	December	7.5	235	42	66	75
	March	7.4	31	9	13	41
5	December	8.0	247	54	70	90
6	December	7.5	251	32	69	79
	March	8.5	58	12	16	63
7	March	9.2	38	7	1	58

Site K.—Benfleet, Essex (London Clay)

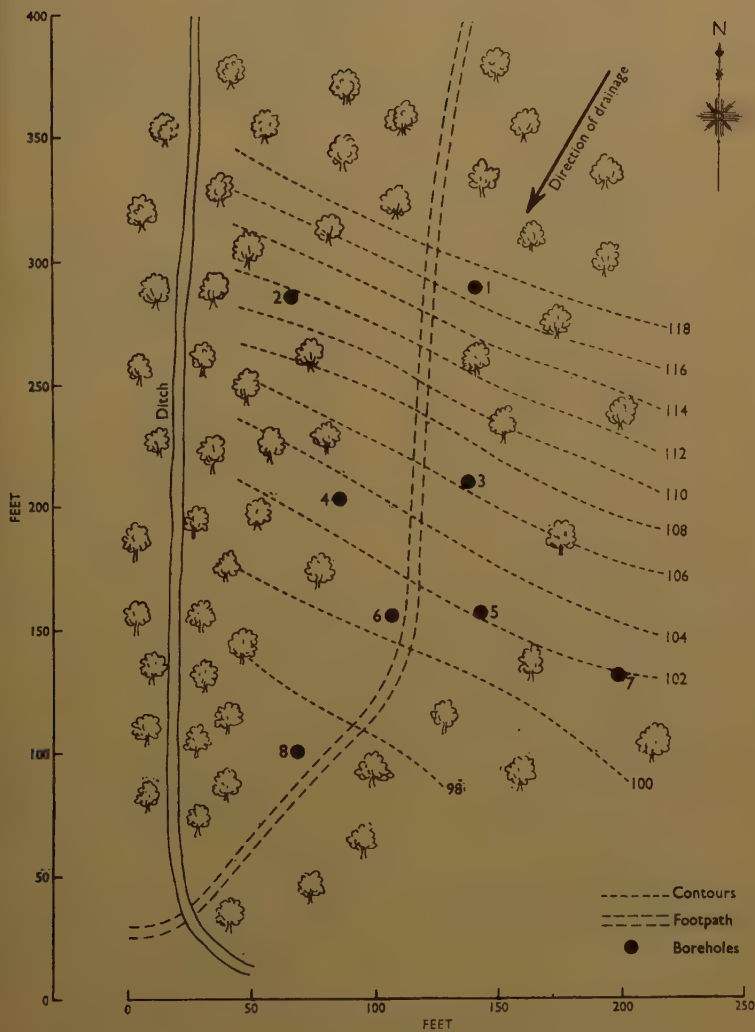
The site at Thundersley Glen, Benfleet, Essex, a plan of which is shown in *Fig. 2*, slopes steeply to the south, with an average gradient of 1 in 10. The subsoil was found to be mainly London Clay, with some patches of drift on the surface. At the lower end of the site there is a small stream and alluvial flat, with alluvium to a depth exceeding 10 feet. At a level higher than the site surveyed, the clay is overlain with sand, and drainage from this sand causes a considerable flow of water down the lower slopes during rainy periods. The portion of the site surveyed measured about 200 feet by 150 feet.

Eight boreholes were sunk to a depth of 10 feet, and were positioned as shown in *Fig. 2*. The groundwater levels were taken 24 hours after boring (except in Nos 7 and 8, which were taken after only 3 hours) early in December, after a period of comparatively dry weather, and again early in March after a wet period. The levels are shown in Table 2.

The water from a number of the boreholes was sampled on each occasion, and the results of the analyses are given in Table 3. The sulphate content of water from the different boreholes in December (dry weather) is uniformly high; in March (wet weather) it is much lower and is rather more variable. The chloride/sulphate ratios, magnesia/lime ratios, base (CaO + MgO)/acid (SO₄ + Cl) ratios, and pH values all tend to be higher in the March samples. These differences are, no doubt, attributable largely to the slower rate of solution of the calcium sulphate and to the relatively higher bicarbonate contents likely to occur in wet weather.

The description of subsoil found in some typical borings is given in Table 4.

Fig. 2



SITE K, BENFLEET, ESSEX

TABLE 4

Boring No. 1	(1) 0 in.-6 in. (2) 6 in.-3 ft 1 in. (3) 3 ft 1 in.-6 ft (4) 6 ft-6 ft 10 in. (5) 6 ft 10 in.-7 ft 9 in. (6) 7 ft 9 in.-10 ft	Topsoil Calcareous clay Brown clay with some sand. Minute gypsum crystals visible Yellow clay. Gypsum crystals visible Brown and blue clay. Gypsum crystals, some of large size Similar to (5). Sandy layer at 9 feet
Boring No. 2	(1) 0 in.-1 ft (2) 1 ft-3 ft 1 in. (3) 3 ft 1 in.-5 ft 8 in. (4) 5 ft 8 in.-8 ft 2 in. (5) 8 ft 2 in.-10 ft	Topsoil Brown clay Brown clay. Gypsum crystals visible Brown clay with sandy pockets. Large masses of gypsum crystals Brown silty clay, with sand and some blue clay and gypsum crystals
Boring No. 3	(1) 0 in.-6 in. (2) 6 in.-3 ft 4 in. (3) 3 ft 4 in.-6 ft 6 in. (4) 6 ft 6 in.-10 ft	Topsoil Brown clay Brown clay. Few minute gypsum crystals Brown, rather sandy clay, with some blue clay
Boring No. 4	(1) 0 in.-6 in. (2) 6 in.-3 ft (3) 3 ft-5 ft 9 in. (4) 5 ft 9 in.-7 ft 11 in. (5) 7 ft 11 in.-10 ft	Topsoil Brown calcareous clay Brown clay. Gypsum crystals visible Brown clay with masses of gypsum crystals Brown and blue clay
Boring No. 7	(1) 0 in.-8 in. (2) 8 in.-3 ft (3) 3 ft-5 ft (4) 5 ft-10 ft	Topsoil Brown clay Brown clay with minute gypsum crystals Brown clay, with blue clay and sand. Gypsum crystal masses at intervals
Boring No. 8	(1) 0 in.-4 in. (2) 4 in.-3 ft 6 in. (3) 3 ft 6 in.-6 ft 3 in. (4) 6 ft. 3 in.-7 ft 10 in. (5) 7 ft 10 in.-10 ft	Topsoil Brown clay Grey black mud with organic matter. Isolated clusters of gypsum crystals Black clay with organic matter and large masses of gypsum Black and brown clay

Positive identification of drift and disturbed clays on London Clay is difficult, but it is believed that most of the borings encounter undisturbed London Clay below about 3 feet. The calcareous clay nearer the surface, and possibly some of the upper brown clays, are of drift origin.

A number of the clay samples were analysed. The water-soluble determinations showed the magnesium sulphate content to vary from a trace up to 0.28 per cent SO_3 , and to average about one-third of the total, whilst the excess of water-soluble sulphate above the water-soluble lime plus magnesia contents, which may be assumed to be alkali sulphate, varied from zero to 20 per cent of the total sulphate content, with a maximum value of 0.12 per cent SO_3 . The total (acid-soluble) sulphate contents varied from 0.02 to 2.82 per cent as shown in Table 5. For convenience,

TABLE 5.—SULPHATE CONTENT OF CLAY SUBSOIL, SITE K, BENFLEET

Borehole No.	Sulphate content (percentage of SO_3)					
	Top of bore (1 to 3 feet depth)		Middle of bore (3 to 6 feet depth)		Bottom of bore (6 to 9 feet depth)	
1	0.05	(2)*	0.05	(3)	0.69	(4-5)
2	0.14	(2-3)	0.31	(4)	1.16	(5)
3	0.03	(2)	0.03	(3)	0.06	(4)
4	0.31	(2)	0.43	(3)	2.82	(4)
7	0.05	(2)	0.18	(3)	0.37	(4)
8	0.02	(2)	0.35	(3)	0.62	(4)

* Bracketed figures are sample numbers, referring to the description of borings given in Table 4.

depths are grouped in three zones in this Table, but the actual depths can be obtained from the description of the borings given earlier and the reference numbers. The results show a wide variation in sulphate content, which on the site generally is high; yet if samples from borehole No. 3 only were taken, the amount found would be small; if samples only to a depth of about 6 feet were taken the amount in No. 1 borehole would also appear low, although in the next few feet a high value is found. In the shallower samples, down to about 3 feet, most of the boreholes show only low sulphate values.

The distribution of the sulphate over the area is irregular, and the amount found in the recent alluvial mud deposit in borehole No. 8 is as high as in much of the older clay deposit. Severe deterioration of concrete has been observed in the Benfleet district.

Site L.—Benfleet, Essex (*Alluvium*)

A site on a marine alluvial marsh at Benfleet, Essex, was also examined. Only one borehole was made, since the site was a small one.

Below the topsoil was found about 20 inches of brown clay, followed by a grey clay to a depth of about 4 feet with patches of brown material, and below this by clay which passed slowly from grey to blue with increasing depth. The water level rose steadily from a depth below ground level

TABLE 6.—ANALYSES OF SOILS

Site	Location	Sample	Acid-soluble percentage* of sulphur trioxide (SO ₃)	Percentage of water solubles*			
				SO ₃	Lime	Magnesia	Calculated SO ₃ in excess of Ca, Mg sulphates
P	Gotham, Nottinghamshire	Pit No. 1. Soil at 2 feet	22.9	—	—	—	—
		" No. 1. " 5 feet	52.8	—	—	—	—
		" No. 2. " 2-4 feet	11.7	1.64	1.14	Trace	Nil
		" No. 2. " 4-6 feet	4.0	1.42	1.00	"	"
E	Hayden, Cheltenham	Bore No. 1. Soil at 2-3 feet	0.06	0.02	0.06	0.003	Nil (excess CaO = 0.04)
		" No. 1. " 4-5 feet	0.09	0.04	0.04	0.001	Nil (excess CaO = 0.01)
		" No. 2. " 2 feet	0.005	—	0.05	0.002	Nil (excess CaO = 0.05)
		" No. 2. " 3-4 feet	0.06	0.04	0.04	0.001	Nil (excess CaO = 0.01)
		" No. 4. " 2 feet	0.03	0.03	0.10	0.004	Nil (excess CaO = 0.08)
		Soil at 1-2 feet	0.06	0.06	0.05	Trace	Nil
Q	Gillingham, Dorset	" 2-4 feet	0.11	0.10	0.08	Trace	Nil
		" 4-6½ feet	21.11	2.42	1.62	0.03	0.05

TABLE 6.—continued

Site	Location	Sample	Acid-soluble percentages* of sulphur trioxide (SO ₃)	Percentage of water solubles*			
				SO ₃	Lime	Magnesia	Calculated SO ₃ in excess of Ca, Mg sulphates
C	Bedfords Park, Romford	Bore No. 1. Soil at 1-3 feet	0.06	0.04	0.07	0.01	Nil (excess CaO = 0.04)
		" No. 1. " 3-5 feet	0.11	0.07	0.07	0.01	Nil (excess CaO = 0.02)
		" No. 2. " 1-5 feet	0.04	0.03	0.11	0.06	Nil (excess CaO + MgO = 0.14)
		" No. 2. " 5½-7½ feet	0.14	0.09	0.02	0.02	0.02
		" No. 3. " 2-4½ feet	0.14	0.05	0.11	0.04	Nil (excess CaO + MgO = 0.11)
		" No. 3. " 4½-6 feet	0.07	0.06	0.04	0.11	Nil (excess CaO + MgO = 0.11)
		" No. 3. " 6-8½ feet	0.10	0.10	0.03	0.02	0.02
		" No. 5. " 3-5 feet	0.12	0.09	0.03	0.07	Nil (excess CaO + MgO = 0.05)
		" No. 5. " 6-8½ feet	0.51	0.49	0.16	0.14	0.03
		Soil at 1-4 feet	0.03	0.03	0.02	0.01	Nil
D	Heaton Grange, Romford	" 4-6½ feet	0.03	0.03	0.02	0.01	Nil
		Soil at ½-4 feet	0.10	0.08	0.02	0.03	Nil
L	Benfleet, Essex	" 4-6½ feet	0.45	0.31	0.03	0.04	0.19

* Percentage contents calculated on soil dried at 100°C.

TABLE 7.—ANALYSES OF GROUNDWATERS

Site	Location	Sample	Date of sample	Analysis: parts per 100,000				pH value	Calculated excess or deficiency of acid ($\text{SO}_3 + \text{Cl}$) over base ($\text{CaO} + \text{MgO}$), expressed as Na_2O^* parts per 100,000
				SO_3	Cl	CaO	MgO		
P	Gotham, Nottinghamshire	Pit No. 1 (2 feet)		165	6	not deter- mined	not deter- mined	8.0	—
		Pit No. 1 (5 feet)		180	6	"	"	8.0	—
		Pit No. 2		113	4	97	4.8	7.2	-25
E	Cheltenham	Bore No. 2	26.10.37	218	75	38	39	7.5	+128
		" No. 7	27.10.37	122	15	22	21	7.5	+51
F	"	" No. 1	"	419	58	60	101	8.0	+152
G	"	" No. 1	"	293	36	61	67	7.5	87
A	Ilford	" No. 7	27.9.37	300	24	71	92	7.5	32
		" No. 8	"	322	144	80	139	8.0	69
		" No. 9	"	277	35	70	89	7.5	29
		" No. 10	"	338	28	66	109	7.5	44
		" No. 1	12.11.37	299	116	78	110	7.5	91
		" No. 2	"	158	42	53	49	8.0	36
		" No. 4	"	364	not deter- mined	89	163	8.0	—

TABLE 7.—*continued*

Site	Location	Sample	Date of sample	Analysis: parts per 100,000				pH value	Calculated excess or deficiency of acid ($SO_3 + Cl$) over base ($CaO + MgO$), expressed as Na_2O^* parts per 100,000
				SO_3	Cl	CaO	MgO		
C	Romford (Bedfords Park)	Bore No. 4	22.10.37	236	41	57	89	8.0	17
		" No. 2	12.11.37	251	32	47	90	8.0	30
		" No. 3	"	209	71	64	90	8.0	13
		" No. 5	"	388	114	75	151	7.5	82
		" No. 6	"	331	158	81	146	8.0	77
		" No. 1	21.10.37	190	3	66	51	8.0	Nil
D I	Romford (Heaton Grange) Bracknell (Winkfield)	" No. 2	5.11.37	187	3	68	53	7.5	-10
		" No. 3	"	150	4	60	41	7.5	-10
		" No. 5	"	233	8	67	74	8.0	-2
		" No. 1	2.12.37	73	334	18	37	7.5	267
L	Benfleet, Essex								

* Assuming that chlorides and sulphates which are not present as Ca or Mg salts, are present as sodium salts.

TABLE 8.—SULPHATE SOIL CONDITIONS AFFECTING CONCRETE, AND RECOMMENDED PRECAUTIONARY MEASURES

Classification of soil conditions			Precautionary measures	
Class	Sulphur trioxide in groundwater: parts SO ₂ per 100,000	Sulphur trioxide in clay: percentage of SO ₂ in air-dry clay	Precast concrete products	Cast-in-situ concrete
1	Less than 30	Less than 0.2	No special measures	<p>Buried concrete surrounded by clay</p> <p>No special measures, except that the use of lean concretes (for example, 1:7, or leaner, ballast concrete) inadvisable if SO₂ in water exceeds about 20 parts per 100,000. Where the latter is the case Portland-cement mixes not leaner than 1:2:4, or, if special precautions are desired, pozzolanic cement, or sulphate-resisting Portland-cement mixes not leaner than 1:2:4 should be used.</p> <p>Concrete exposed to one-sided water pressure, or concrete of thin section</p> <p>No special measures, except that when SO₂ in water exceeds 20 parts per 100,000, special care should be taken to ensure the use of high-quality P.C. concrete, if necessary 1:1½:3 mixes; alternatively, pozzolanic cements or sulphate-resisting Portland cement may be used in mixes not leaner than 1:2:4.</p>
2	30 to 100	0.2 to 0.5	Rich Portland-cement concretes, for example, 1:1½:3, are not likely to suffer seriously except over a very long period of years. Alternatively, either pozzolanic, sulphate-resisting Portland cement, high-alumina or supersulphate cement should be used.	<p>Rich P.C. concretes, e.g. 1:1½:3, are unlikely to suffer seriously over short period of years, provided care is taken to ensure that a very dense and homogeneous mass is obtained. For most work, particularly if predominant salts are magnesium or sodium sulphates, concrete made with either pozzolanic cement, or supersulphate cement, or high-alumina cement (not leaner than 1:2:4) or high-alumina cement (1:2:4) is advisable.*</p> <p>The use of Portland-cement concrete is not advisable. Pozzolanic cement or sulphate-resisting Portland cement, or preferably either high-alumina cement or supersulphate cement is recommended.</p>

TABLE 8—*continued*

Classification of soil conditions			Precautionary measures	
Class	Sulphur trioxide in groundwater: parts SO ₂ per 100,000	Sulphur trioxide in clay: percentage of SO ₃ in air-dry clay	Precast concrete products	Cast-in-situ concrete
3	More than 100	More than 0.5	<p>The densest Portland-cement concrete is not likely to suffer seriously over periods of up to, say, 10–20 years, unless conditions are very severe. Alternatively, sulphate-resisting, high-alumina, or supersulphate-cement concretes should be used.</p>	<p>Buried concrete surrounded by clay</p> <p>Concrete exposed to one-sided water pressure, or concrete of thin section</p> <p>The use of high-alumina or supersulphate-cement concrete is recommended.</p>

* Where 1 : 2 : 4 concrete is mentioned, other mixes of equivalent weight ratio of cement to total aggregate, but with somewhat increased ratio of sand to coarse aggregates (for example, 1 : 2½ : 3½ or even 1 : 2½ : 3½) may be used, sometimes with advantage. It may be necessary when using supersulphate cement to employ mixes somewhat richer than 1 : 2 : 4 in order to obtain adequate workability.

of 8 feet (when bored) to 3 feet (24 hours later), and to 15 inches (48 hours later).

Analyses of the soil and water are given in Tables 6 and 7. They differ completely from the analyses shown for other types of site, owing to the presence of a large proportion of sodium salts, as might be expected on a marsh of this type. The low content of water-soluble calcium and magnesium in the deeper clay sample, in relation to the sulphate content, is also rather remarkable.

Site I.—Bracknell, Berkshire (London Clay)

A site near Bracknell, Berkshire, situated adjacent to the sewage treatment works at Winkfield showed brown clay at depths down to 6 or 8 feet, below which grey or blue clay occurred. There were a few very small gypsum crystals in some parts of the brown clay, and large crystals were found at about 10 feet depth in two boreholes. The groundwater levels were low (8 to 10 feet below surface) at the time of boring (4th and 5th November, 1937).

Analyses of the groundwater from three boreholes are shown in Table 7. They show high and somewhat variable sulphate contents present as the calcium and magnesium salts, and very little chloride.

Site T.—Chippenham, Wiltshire (Oxford Clay)

A site at Chippenham, Wiltshire, shown as Oxford Clay on the Geological Survey maps, was given a preliminary examination. Two pits were dug, one to a depth of 6 feet, and the other to 10 feet. Beneath the topsoil a variable brown clay, with some peat, was found to a depth of 4 feet; below this was a thin seam of calcareous sandstone, followed by about 4 or 5 feet of sandy clay. A hard dark-blue clay was found at 9 feet.

The groundwaters contained less than 10 parts per 100,000 of sulphate, and the soil samples, down to 4 feet, less than 0.1 per cent of sulphate. The clays below the calcareous sandstone contained much more sulphate, showing up to about 0.8 per cent in some spot samples.

Site Q.—Gillingham, Dorset (Kimmeridge Clay)

Preliminary samples of soil from a site at Gillingham, Dorset, were examined. The results, given in Table 6, showed that, whilst there was a great deal of gypsum at depths below 4 feet, there was very little at lesser depths. There was no groundwater down to 6 feet 6 inches at the time of boring.

CONCLUSIONS

The results of the survey provide some useful indications for the engineer who wishes to assess the risk of sulphate attack on a site. The following conclusions may be drawn :—

- (1) The sulphate contents of groundwaters taken from boreholes or excavations in clay subsoil may be fairly uniform in a restricted area, but they are liable to wide seasonal variations with rainfall. Following high recent rainfall they may also be more variable from point to point and less reliable as an indication of the general sulphate content of the subsoil in the area.
- (2) The results of analyses of the subsoil clay may vary widely with depth from the surface, and from point to point even in a very small area.
- (3) The results of analyses of soil samples give very inadequate guidance on the risk of sulphate action on concrete, because of the variations mentioned under (2), and because the groundwaters, through which the attack occurs, may be derived from ground some distance away from the point of sampling, and may thus collect sulphates from quite different soils. A low sulphate figure determined in the soil sample, in a district known to have, or suspected of having, clay subsoils with high sulphate contents, cannot be taken as sufficient evidence that there is no risk of attack on ordinary Portland-cement concrete.
- (4) Sulphate contents are usually low near the surface and, in sulphate-bearing soils in Great Britain, increase progressively with increasing depth. On well drained soils, concrete placed near the surface may often, therefore, be expected to have a normal life, even though the amount of sulphate at greater depths is high.

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The Paper is accompanied by three sheets of diagrams, from which the figures in the text have been prepared, and by the following Appendices.

APPENDIX I

RECOMMENDATIONS FOR SAMPLING IN ASSESSING THE RISK OF SULPHATE ACTION ON CONCRETE

Where a site in an area suspected of having appreciable sulphate in the soil is being examined to assess the risk of sulphate action on concrete, it is first desirable to study the geological maps and any available records of shallow borings or excavations in the vicinity, and to consider the contours and probable direction of groundwater drainage. Such a preliminary examination may sometimes be sufficient to show that the probability of trouble is slight, and one or two bores may be sufficient to confirm this. The season and recent rainfall should also be taken into consideration, both in deciding what samples may be necessary and in interpreting the results of analyses.

The number of trial holes or borings required will vary with the conditions and cannot always be estimated in advance. More are required when the holes are dry (and only soil samples can be examined) than when groundwater is found; it may also be necessary to have more when variable results are obtained from one to another. Under the more adverse conditions it is not advisable to rely upon less than three holes on a small site, six holes per acre on larger sites, and ten per mile along sewer lines; but if the first few borings, suitably distributed, give groundwater of low sulphate content, then the number may be reduced.

Boreholes made with a post-hole auger, as was used in the present work, are very easily made through loam, sand, or clay and require very much less work than excavating trial holes. Water samples can be taken readily from a hole of this diameter by a weighted bottle or can on a string, or a proper water-sampling vessel of suitable diameter. All bores or holes should be taken down to the maximum depth of the proposed works.

All samples of water should be placed in thoroughly clean bottles which have been rinsed out with distilled water. For field tests less than $\frac{1}{2}$ pint of water is sufficient, but for complete laboratory tests at least 1 pint is required. The sample should be taken from water actually draining from the surrounding subsoil into the borehole, pit, or trench; fresh rainwater which has fallen directly into these, or run in from the surface, is of no value, and it is preferable to avoid taking samples immediately after heavy rainfall.

The sides of excavations, or the cores from borings, should be examined before taking samples to ascertain the variations in character with depth. The positions of any water-bearing layers should be noted and the soil should be examined for the presence of calcium sulphate (gypsum) crystals. These may occur as large rounded agglomerations of radiating colourless crystals, or as small crystals scattered in the clay; these small crystals can often be discerned by their shiny appearance when a piece of clay is held up in the light. Other minerals occurring as colourless crystals are not likely to be found in clay, and the identity of any found can be checked by their softness, since gypsum crystals can be scratched by the finger nail. If gypsum crystals are found their distribution should be carefully noted.

The individual soil samples, several pounds in weight and consisting of cores or scrapings from the sides of excavations, should generally represent intervals of 2 to 3 feet of depth, or less if obvious changes in soil character occur. These samples should be separately mixed up by battering down and breaking up repeatedly with a spade or small shovel until apparently uniform, and quartered or reduced following standard sampling procedures until about 1 lb. is left if field testing only is to be done, or 2 to 3 lb. if samples are to be sent to the laboratory.

Field testing

If the site is a large one it may often be an advantage to carry out rough site tests to determine whether or not laboratory tests are necessary and, if so, which samples need to be sent to the laboratory. The following simple tests are sufficient to show very roughly if the samples contain less than a certain minimum amount of sulphates, below which further testing may for most work be regarded as unnecessary. Values of 0.02 gramme of SO_2 per 100 cubic centimetres for groundwater samples, and 0.15

per cent SO_3 for soils are the limits adopted in the test as described below, but the tests can, of course, be modified for any other limiting figures to which it is desired to work.

The equipment required for testing is simple and may readily be contained in a small case fitted with clips to hold the various items, which are as follows:—

Six 6-inch by 1-inch glass boiling tubes, graduated with marks at approximately 5, 10, 15, 20, 25, and 30 cc.

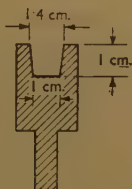
One small glass funnel (about 2 inches top diameter).

One box 7 cm. coarse filter papers (for example, No. 1 Whatman filter paper or similar).

One small spirit lamp.

One small measure, capacity 1.1 cc.; this may conveniently be drilled from a piece of brass rod as in *Fig. 3*.

Fig. 3



1.1-CC. MEASURE

One small glass or porcelain pestle and mortar of $2\frac{1}{2}$ to 3 inches diameter.

Solution A.—One small bottle (8 oz.) of hydrochloric acid of twice normal concentration.

„ B.—One small bottle (8 oz.) of barium chloride solution of 0.2 normal concentration.

„ C.—One small bottle (8 oz.) of a standard sulphate solution containing 0.02 gm. SO_3 per 100 cc. This may be prepared by dissolving 0.62 gm. crystallized magnesium sulphate ($\text{MgSO}_4 \cdot 7\text{H}_2\text{O}$) in 1 litre of distilled water.

One small bottle of distilled water, preferably fitted with a jet.

In testing a sample of groundwater, the sample should, if turbid, first be filtered into one of the boiling tubes. The clear water is then poured into one tube up to the fifth mark (25 cc.) and the standard sulphate solution (C) into a second tube up to the same mark. Both are then made up to the sixth mark (30 cc.) with the hydrochloric acid solution (A) and 10 drops of barium chloride solution (B) are added to each. After shaking, in order to mix the solutions thoroughly, the amount of turbidity or precipitate in the water sample is compared with that in the standard; if the amount is equal to or greater than that in the standard, the water is to be regarded as sufficiently aggressive to require further investigation in the laboratory.

In testing soils the procedure is rather less simple, since any sulphate present has first to be brought into solution. From the 1 lb. sample, sufficient is taken to half fill the small mortar, and a little water added if necessary to bring the material to a butter-like consistency on grinding with the pestle. The grinding is continued until the sample is thoroughly uniform and free from coarse material; any pebbles may be removed by hand. The standard measure (*Fig. 3*) is filled to the top with the prepared sample and the latter then transferred to a boiling tube with the aid of a spatula or penknife. The measure may be washed with a few drops of water and the washings transferred to the boiling tube. About 10 cc. of hydrochloric acid solution (A) (two divisions on the tube) are added and the contents of the tube brought to boiling point over the spirit lamp. The liquid is then filtered through the filter paper and funnel into another boiling tube, and the filter washed once with distilled water. When all the liquid has passed through the filter, the volume is made up to the fourth mark

(20 cc.) with distilled water. Into a further tube the standard sulphate solution (C) is poured up to the third mark (15 cc.), and then hydrochloric acid solution (A) up to the fourth mark (20 cc.). Ten drops of barium chloride solution (B) are then added to both the soil solution and standard sulphate solution prepared in this way. The tubes are shaken to mix the contents thoroughly and the amount of turbidity or precipitate in the solution obtained from the soil sample is compared with the standard. If the amount is equal to or greater than that in the standard the soil is to be regarded as requiring further investigation.

The measure of soil used contains approximately 1.1 cc., which, with a density of about 1.8, is equivalent to approximately 2 grammes of wet soil; 15 cc. of the standard sulphate solution contains 3 milligrams of sulphate (SO_3) and therefore the approximate limit set is 0.15 per cent of acid-soluble sulphate in the wet clay.

APPENDIX II

CONCRETE IN SULPHATE-BEARING SOILS

The measures desirable to protect concrete from deterioration in sulphate-bearing soils vary with the type of concrete and the soil conditions. An arbitrary classification is shown in Table 8 in terms of three classes of sites with low, moderate, and high sulphate content. The groundwater analysis when available is normally to be preferred to the clay analysis as a criterion. The recommendations are to be regarded only as a general guide and they tend to be conservative, since conditions may be more severe than is indicated by the samples taken. In applying them, the engineer will naturally take into account local information and experience and the life desired from the structure.

The least severe condition of exposure to sulphate attack is that of concrete completely buried, under circumstances where the excavation does not form a channel along which flow of groundwater is likely to occur. Foundations of buildings and precast piles will usually fall into this class. The most severe condition arises when concrete is exposed on one side to water pressure and on the other side to air, promoting evaporation. Structures founded near the surface, such as road-slabs and shallow strip foundations, will not usually suffer deterioration from sulphate attack since the sulphate content is usually low in the upper 2 or 3 feet of a clay and the groundwater will only rise to this level in wet seasons when the concentration of sulphates in it tends to fall.

The use of precast concrete is advantageous where practicable, since it can be well matured before exposure to the sulphate waters, and its quality more readily controlled than with in-situ concrete. The densest precast Portland-cement concretes—for example, some types of concrete pipes and piles—have a considerable resistance to attack by sulphates and deterioration is likely to be very slow in concentrations up to, or even greater than, that given by a saturated solution of gypsum (about 125 parts of sulphur trioxide per 100,000 of groundwater). Asbestos-cement pipes have a similarly good resistance.

Lean ballast concretes, cast in situ, of the type often used for haunching and bedding, are very vulnerable to attack. The use of sulphate-resisting Portland cement, or pozzolanic cement, for conditions of moderate severity (such as 100 parts of SO_3 per 100,000, or for saturated solutions of gypsum), and of high-alumina or super-sulphate cement for severe conditions, will help to give durable structures. Strict control of concrete quality is essential, for none of these cements is immune from attack in low quality or lean concretes. Construction joints are particularly vulnerable. Mixes of $1 : 2\frac{1}{2} : 3\frac{3}{4}$ or $1 : 2\frac{1}{2} : 3\frac{1}{2}$ are often to be preferred to a $1 : 2 : 4$ mix to improve workability unless the sand is very fine. Mixes should not be leaner than this and richer mixes may be used, except with high-alumina cement for which appreciably richer mixes are to be avoided. "All-in" aggregates should not be used since their grading is apt to be variable or unsatisfactory, and the quality of the resultant concrete is unlikely to be good enough for the sulphate-resistant qualities of these cements to be properly developed.

In suitable cases surface protection can be given to Portland-cement concrete. For example, two coats of a coal-tar-pitch mix applied hot to a reservoir retaining-

wall before backfilling has given a good performance.⁶ Reliance should not be placed on surface or integral waterproofers to protect concrete against sulphate attack. In pipes, Portland-cement joints have sometimes been protected by sticking strips of bituminous sheeting over them with a bituminous adhesive.⁷ High-alumina cement is to be recommended for brickwork mortar or pipe joints when serious attack by sulphates may occur. With stoneware pipes a 1 : 2½ mix of high-alumina cement and sand (from ⅜-inch mesh down to No. 100 mesh), or a 1 : 2 mix with sand of ⅛ inch or finer is suitable for jointing. Difficulty is sometimes experienced in getting good adhesion between high-alumina cement and glazed stoneware, but pipes can be obtained with the spigot and socket unglazed.

A fuller discussion is to be found in Building Research Station Digest No. 31, on concrete in sulphate-bearing clays and groundwaters.*

⁶ The references are given on p. 177.

Paper No. 5877

“Numerical Solution of Some Problems in the Consolidation of Clay”

by

Robert Edward Gibson, B.Sc. (Eng.), and Peter Lumb, M.Sc. (Eng.), Studs I.C.E.

(Ordered by the Council to be published with written discussion)†

SYNOPSIS

The rate of consolidation settlement of structures founded above clay strata is usually calculated on the simplifying assumption that the flow of pore-water takes place in one direction only. Although it is widely appreciated that the flow is generally three-dimensional, exact mathematical solutions to problems have been obtained in very few cases which are of importance to the civil engineer.

In this Paper a “step-by-step” method for a solution of the governing equation is presented and used to solve a number of problems. Exact solutions are known to the simpler of these, which fact enables a comparison to be made with the solution as obtained by the numerical procedure. To the more involved problems concerning the rate of settlement of a uniformly loaded circular footing exact solutions are difficult to obtain but the method outlined has enabled a solution, sufficiently accurate for all engineering purposes, to be rapidly evaluated. This solution indicated that the rate of settlement is, in fact, appreciably greater than that found from the simple one-dimensional theory.

INTRODUCTION

IN the theory of consolidation of homogeneous isotropic clay which was first formulated by Professor Karl von Terzaghi in 1925,¹ the simplifying assumption was made that the flow of pore-water takes place in one direction only. This is a condition which would obtain in practice if a stratum of clay were uniformly loaded over its surface. This one-dimensional consolidation process is governed by an equation ‡ of the form :

$$c_v \frac{\partial^2 u}{\partial z^2} = \frac{\partial u}{\partial t} \quad \dots \dots \dots (1)$$

The solution of this equation, in any particular case, requires that the position of the drainage surfaces and the stress distribution in the layer

† Correspondence on this Paper should be received at the Institution by the 1st July, 1953, and will be published in Part I of the Proceedings. Contributions should be limited to about 1,200 words.—SEC. I.C.E.

¹ The references are given on p. 196.

‡ For notation, see Appendix I.

should be known. Solutions for a number of cases have been given by Terzaghi and Fröhlich (1936). The validity of applying this simple theory to determine the rate of settlement of a foundation will depend on the extent to which the condition of one-dimensional flow is satisfied. In many cases it is not even approximately satisfied and in recognition of this limitation the consolidation equation is modified to become :

$$\frac{\partial u}{\partial t} = c \left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} \right) \quad . \quad . \quad . \quad (2)$$

this equation being assumed to hold so long as the foundation loads do not vary with time.

A general theory of consolidation, which showed that the stress distribution and the consolidation process are interconnected, was developed by Biot.² From this theory the Authors have shown (Appendix II) that the consolidation process may be described by the equation :

$$-\frac{1}{3} \frac{\partial \theta}{\partial t} + \frac{\partial u}{\partial t} = c \left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} \right) \quad . \quad . \quad . \quad (3)$$

where θ denotes the sum of the normal total stresses ($\sigma_{xx} + \sigma_{yy} + \sigma_{zz}$) at any point. It is seen that equation (2) is a good approximation to this equation, for, when the foundation loads are constant, it may be shown that the variation of θ with time is generally very small, arising only from the changes during consolidation of Poisson's Ratio and other parameters of the clay with respect to the total stresses.

Equations, similar in form to those above, describe the flow of heat in a homogeneous solid, but although exact solutions to a great variety of problems in this field have been published,³ few are of direct value to foundation engineers. However, two numerical methods of solving such equations have been developed^{4,5} which are suitable for application to consolidation problems.

In this Paper a numerical method, which is similar to that given in reference 4, will be used in solving equation (2) for a number of different cases, comparisons being made with exact solutions where they are available. This method is capable of extension to cases where c , the coefficient of consolidation is not constant, and where the boundary conditions are more complicated, but such extensions will not be considered here.

ONE-DIMENSIONAL CONSOLIDATION

As an illustration of the method, the simplest of the cases of one-dimensional consolidation will be discussed.

A uniform vertical pressure u_0 is applied to a laterally confined cylinder of clay, of thickness $2H$, which is permitted to drain freely from its horizontal surfaces. Consolidation takes place and after a certain time, largely depending upon the thickness of the sample, it practically ceases and the

sample can then be considered to be in equilibrium. If the final compression of the sample is denoted by S_∞ (since theoretically an infinite time is required for equilibrium to be reached) and the compression at any time t by S_t , then the average degree of consolidation \bar{U} at this time is defined by the equation:—

$$S_t = \bar{U} S_\infty$$

In order to determine the relationship between the degree of consolidation \bar{U} and the time, it is necessary to consider the equation:

$$c_v \frac{\partial^2 u}{\partial z^2} = \frac{\partial u}{\partial t}$$

which describes the variation with z and t of the pore-water pressure u within the sample.

The solution of this equation, with the boundary conditions:

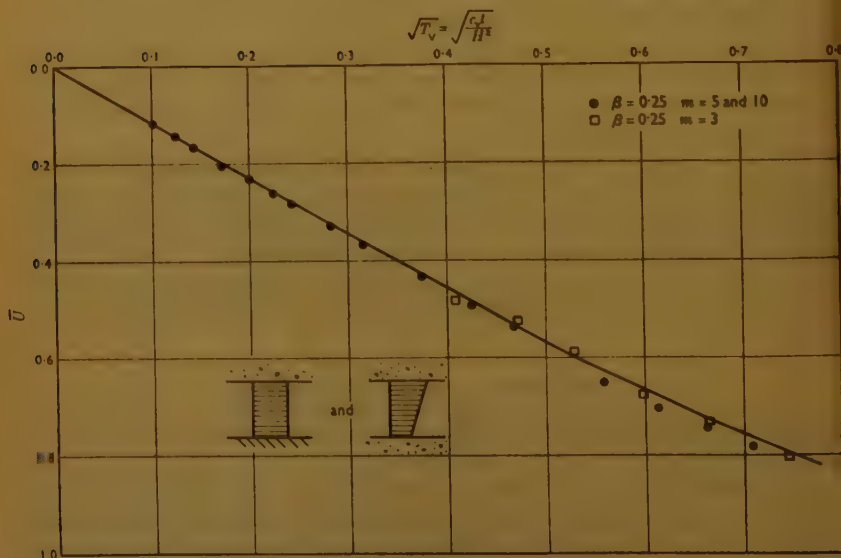
$$u = 0 \text{ at } z = 0 \text{ and } 2H, \text{ when } t > 0$$

$$u = u_0 \text{ at } 0 \leq z \leq 2H, \text{ when } t = 0,$$

has been given by Terzaghi and Fröhlich⁶ in the form:

$$u = \frac{4}{\pi} u_0 \sum_{n=0}^{\infty} \frac{1}{(1+2n)} \sin \left(\frac{(1+2n)\pi z}{2H} \right) e^{-(2n+1)^2 \frac{\pi^2}{4} T_v}$$

Fig. 1



where $T_v = \frac{c_v t}{H^2}$ is a dimensionless independent variable called the "time factor."

If it is assumed that the compression depends linearly upon the effective stress change ($u_o - u$), it follows that the degree of consolidation is given by :

$$\bar{U} = \frac{\int_0^{2H} (u_o - u) dz}{\int_0^{2H} u_o dz} \quad . \quad . \quad . \quad . \quad (4)$$

$$\text{or: } \bar{U} = 1 - \frac{8}{\pi^2} \sum_{n=0}^{\infty} \frac{1}{(1+2n)^2} e^{-(1+2n)^2 \frac{\pi^2}{4} T_v} \quad . \quad . \quad . \quad . \quad (4a)$$

This relationship between \bar{U} and T_v is shown in *Fig. 1*.

THE NUMERICAL METHOD

Consider a rectangular region (*Fig. 2*), every point of which is specified by co-ordinates z and t . A rectangular mesh of sides δt and δz , extending over the whole region, is constructed and, instead of considering the values of u at all the points of the region, only those at the mesh points such as 0, 1, 2, 3, and 4, are considered. (See *Fig. 3*.) At the point 0 the pore-water pressure must satisfy equation (1) :

$$\left(\frac{\partial u}{\partial t} \right)_o = c_v \left(\frac{\partial^2 u}{\partial z^2} \right)_o$$

and since* :

$$\left(\frac{\partial^2 u}{\partial z^2} \right)_o \simeq \frac{u_2 + u_4 - 2u_o}{\delta z^2}$$

and :

$$\left(\frac{\partial u}{\partial t} \right)_o \simeq \frac{u_1 - u_o}{\delta t}$$

it follows that :

$$u_1 \simeq \beta (u_2 + u_4 - 2u_o) + u_o \quad . \quad . \quad . \quad (5)$$

where

$$\beta = \frac{c_v \delta t}{\delta z^2}$$

Since the boundary and initial conditions have been specified it will be seen that a successive application of equation (5) to mesh points proceeding from smaller to larger times will give the value of u at all mesh points (see *Fig. 2*).

The accuracy of the solution will depend upon the number of intervals

* For a discussion of finite difference approximations to derivatives see reference 7.

Fig. 2

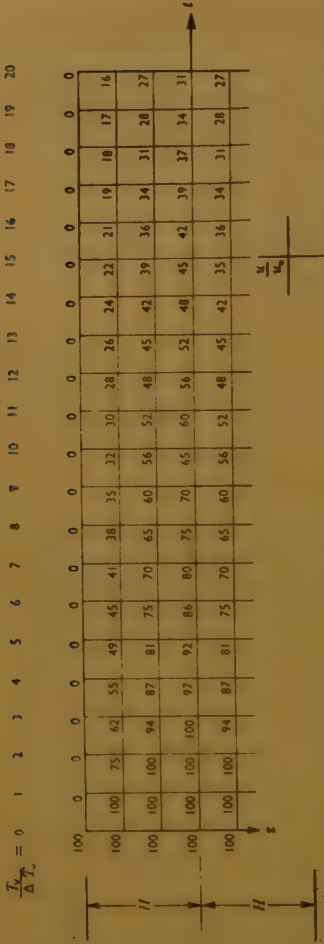


Fig. 3



$2m$ ($= 2H/\delta z$) into which the thickness of the sample has been divided, and upon the value chosen for β . Smaller values of β and larger values of m will correspond to more exact solutions, although the work involved will be proportionately increased. If $t = n\delta t$, then the time factor can be evaluated from the expression :

$$T_v = \frac{n}{m^2} \cdot \beta$$

Suppose an initial pressure of 100 units is applied to the sample then during the time interval 0 to δt the pore-water pressure on the boundaries drops from 100 units to zero, whilst the succeeding values of u at all the

internal mesh points are calculated using equation (5) with values of $\beta = 0.25$ and $m = 3$. To increase the accuracy of the solution the calculation was repeated with $\beta = 0.25$, first with $m = 5$ and then with $m = 10$. The results of these calculations are shown in *Fig. 1* where they are compared with the exact solution for the relation with \bar{U} and T_v as found from equation (4a). It will be seen that over the range $0 < \bar{U} < 0.8$ the agreement between the numerical method and the exact solution is sufficiently close for practical purposes.

This numerical method has, however, the disadvantage that it is difficult to determine the pore-water pressure-distribution accurately after long periods of time. This is rarely required in practice, but by the use of relaxation methods⁵ it could be determined without excessive labour.

CONSOLIDATION OF A CYLINDER OF CLAY

Consideration will now be given to problems which possess axial symmetry. The first cases considered are those relating to the consolidation of a cylinder of clay. Here again, exact solutions exist and the object of applying the numerical method is to examine its reliability in such cases.

If an all-round pressure u_o is applied to a cylindrical sample of clay, of radius R and length $2H$, which is permitted to drain radially, the equation of consolidation is most conveniently written in terms of polar co-ordinates :

$$\frac{\partial u}{\partial t} = c \left(\frac{\partial^2 u}{\partial r^2} + \frac{1}{r} \frac{\partial u}{\partial r} \right) \quad \dots \quad (6)$$

From a solution given by Carslaw and Jaeger,³ relating to heat flow, it may easily be shown that the average degree of consolidation is given by

$$\bar{U} w_n^2 = 1 - 4 \sum_{n=1}^{\infty} \frac{1}{w_n^2} \cdot e^{-\frac{ct}{R^2} w_n^2} \quad \dots \quad (7)$$

where w_n is the n th root of the equation $J_o(w) = 0$, J_o denoting the Bessel function of order zero.

From this relation, \bar{U} as a function of $T_R = \frac{ct}{p^2}$ has been plotted in *Fig. 4*.

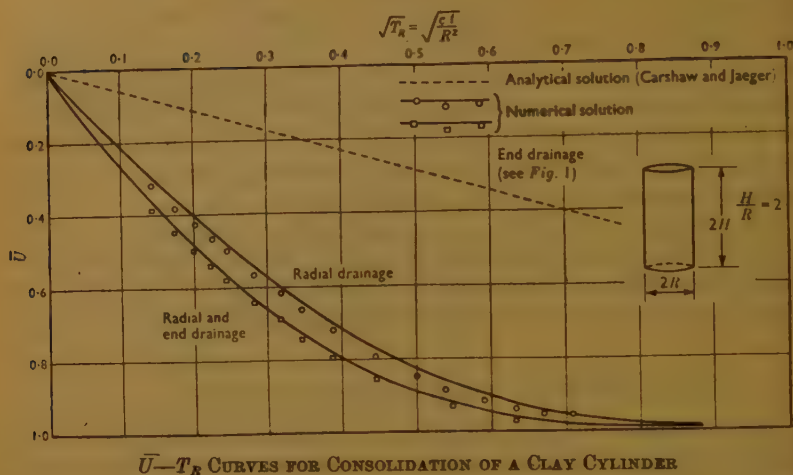
To obtain a numerical solution, equation (6) is written in the form :

$$u_4 \approx \beta \left[u_1 + u_3 - 2u_o + \frac{(u_1 - u_3)}{2p} \right] + u_o \quad \dots \quad (8)$$

$$\text{where } \beta = \frac{c\delta t}{\delta r^2} \text{ and } p = \frac{r}{\delta r},$$

by making use of the usual approximations.

Fig. 4



This equation cannot be used on the axis $r = 0$ where $p = 0$, and it is necessary only to note that as $r \rightarrow 0$:

$$\frac{1}{r} \frac{\partial u}{\partial r} \rightarrow \frac{\partial^2 u}{\partial r^2}$$

so that on the axis $r = 0$:

$$u_4 = 2\beta(u_1 + u_3 - 2u_0) + u_0 \quad \dots \quad (9)$$

In order to extend the solution to a large range of time with the minimum of labour, two cases were worked out in which:

$$(1) \quad \frac{R}{\delta r} = 5, \beta = 0.25$$

$$\text{and } (2) \quad \frac{R}{\delta r} = 2, \beta = 0.20$$

the values of \bar{U} being calculated from:

$$\bar{U} = \frac{\int_0^R (u_0 - u) r \cdot dr}{\int_0^R u_0 r \cdot dr}$$

which are shown in Fig. 4.

The comparison between this numerical solution and the exact solution is reasonably good, although the values of \bar{U} obtained from the numerical method are consistently rather higher than the exact values, the maximum error in \bar{U} being about 4 per cent.

A more involved case is that in which a cylinder is permitted to drain

freely from all its surfaces. The equation which must be satisfied may be written in cylindrical co-ordinates :

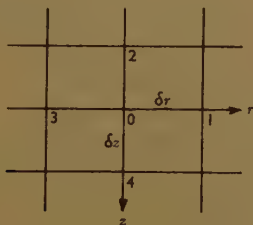
$$\frac{\partial u}{\partial t} = c \left(\frac{\partial^2 u}{\partial r^2} + \frac{1}{r} \frac{\partial u}{\partial r} + \frac{\partial^2 u}{\partial z^2} \right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

It has been shown by Carrillo⁸ that this three-dimensional radial flow may be resolved into a plane radial flow (equation (6)) and a linear flow (equation (1)), and if \bar{U}_R and \bar{U}_v respectively are the degrees of consolidation due to these flows, then the degree of consolidation in three-dimensional axially symmetrical flow \bar{U} is given by :

$$\bar{U} = 1 - (1 - \bar{U}_R) (1 - \bar{U}_v) \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

From this equation the variation of \bar{U} with the time factor $T_R = \frac{ct}{R^2}$ has been computed using equations (4a) and (7) (see *Fig. 4*) for a cylinder with $H/R = 2$.

Fig. 5



Since three co-ordinates r , z , and t occur in equation (10) it becomes necessary to associate with each node of the network a series of values of the pore-water pressure, showing its variation with time, the co-ordinates r and z occupying the plane of the paper. (See *Fig. 5*.)

The equation (10), written in finite difference form, becomes :

$$u_o(t + \delta t) \doteq \beta \left[u_1 + u_2 + u_3 + u_4 - 4u_o + \frac{u_1 - u_3}{2p} \right] + u_o \quad . \quad . \quad (12)$$

where $p = \frac{r}{\delta r}$, $\beta = \frac{ct}{h^2}$, and $\delta r = \delta z = h$

and on the axis it takes the form :

$$u_o(t + \delta t) \doteq \beta (2u_1 + u_2 + 2u_3 + u_4 - 6u_o) + u_o \quad . \quad . \quad . \quad (13)$$

For a cylinder in which the ratio of the length $2H$ to the diameter $2R$ was 2, two cases were worked out in which :

$$(1) \quad \frac{R}{\delta r} = 5, \quad \beta = 0.05 \text{ and } 0.25$$

$$\text{and } (2) \frac{R}{\delta r} = 2, \quad \beta = 0.2 \text{ and } 0.4$$

the values of \bar{U} being calculated from the equation :

$$\bar{U} = \frac{\int_0^{2H} \int_0^R (u_0 - u) r dr dz}{\int_0^{2H} \int_0^R u_0 r dr dz}$$

and compared with the exact solution in *Fig. 4* as obtained from Terzaghi's linear flow solution and Carslaw and Jaeger's radial flow solution, together with equation (11). The agreement is seen to be good.

CONSOLIDATION IN CLAY BENEATH A CIRCULAR FOOTING

Having shown in three cases, involving one- and two-dimensional consolidation processes, that the numerical method yields results in close agreement with the exact solutions, the numerical method may now be applied to problems for which no exact solution exists, and for which an exact solution would be difficult to obtain and evaluate.

Two cases of some practical interest will be considered, namely, the consolidation of a clay layer beneath a uniformly loaded circular flexible footing ; first, with a footing very permeable in comparison with the clay, and secondly with an impermeable footing.

In order to estimate the time/settlement curve in such cases it has been usual to employ the one-dimensional consolidation equation (1), but it is evident that appreciable error may thereby be introduced. The three-dimensional analytical solution of such problems presents great difficulties, whilst the numerical solution involves little more labour than that required in the previous example.

Considering now the case of the permeable footing, the pore-water pressure u must satisfy equations (12) and (13) together with the boundary conditions :

$$\begin{aligned} z = H, \quad \frac{\partial u}{\partial z} &= 0, t > 0 \\ z = 0, \quad u &= 0, t > 0 \end{aligned}$$

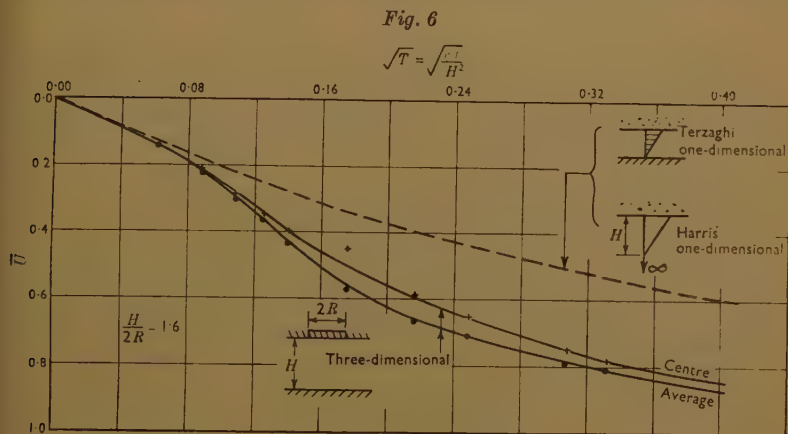
the pore-water being free to drain both through the surface of the clay and through the footing.

The initial condition, that is, the value of the pore-water pressure immediately after the load has been applied, has been assumed to be equal to the major principal stress. This assumption is in accord with the limited evidence available, and is moreover a consequence of Skempton's λ -theory,⁹ which predicts that the initial pore-water pressure, before any drainage has had opportunity to occur, is given by :

$$u_0 = \frac{\sigma_1 + \lambda(\sigma_2 + \sigma_3)}{1 + 2\lambda}$$

where σ_1 , σ_2 , and σ_3 denote the major, intermediate, and minor principal stresses at a point and λ is a parameter characterizing the behaviour of the clay. For most clays it appears that λ has small values (about 0.05 to 0.30) and the assumption made above is equivalent to taking λ as being zero.

The rate of consolidation of the centre of the loaded area was evaluated in a case where the ratio of the depth of the clay stratum to the diameter of the loaded area was 1.6, the solution being shown in *Fig. 6*. The initial pore-water pressure, assumed to be equal to the major principal stress, was evaluated from Tables prepared by Jürgenson¹⁰ from Boussinesq's solution. This solution is compared with solutions of the one-dimensional



equation derived by Terzaghi and Fröhlich⁶ and by Harris,* where the initial pressure in the pore-water is distributed triangularly with depth, the depths of the stratum being assumed to be finite and infinite respectively in these two cases. For about the first half of the consolidation process ($\bar{U} < 0.6$) these two theoretical solutions do not differ greatly and thus it may be inferred that the numerical solution of the problem which includes radial flow would hold, at least for short times even if the thickness of the clay stratum were somewhat greater than the $1.6 \times 2R$ adopted.

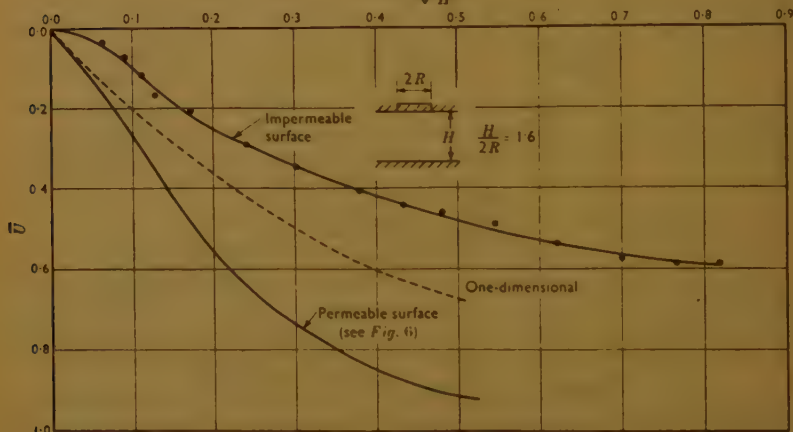
Turning now to the comparison between this numerical solution and the one-dimensional solution (see *Fig. 6*), it will be seen that, except for small times ($\bar{U} < 0.2$) the consolidation with three-dimensional flow is decidedly more rapid, as would be expected.

* A private communication from A. J. Harris. The work was carried out at the Building Research Station (Department of Scientific and Industrial Research) and the results are given with permission from the Director of Building Research.

A further problem of interest is that of determining the rate of consolidation of the clay beneath an impermeable circular footing founded at considerable depth. An approximate analysis of this problem has been carried out by replacing the permeable upper surface of the clay in the previous case by an impermeable surface. Since the pore-water is not permitted to escape, a redistribution takes place in the stratum, the pore-water flowing away from regions of high stress intensity around the buried footing. As a consequence of this redistribution, swelling takes place in regions away from the footing and a coefficient of rate of swelling, somewhat larger than c , should be used. This factor could be incorporated into the numerical solution of the problem but it was not considered worth while, since the errors involved would probably be small.

Fig. 7

$$\sqrt{T} = \sqrt{\frac{cI}{H^2}}$$



\bar{U} — T CURVES FOR CONSOLIDATION OF CLAY LAYER. CIRCULAR UNIFORM LOAD (IMPERMEABLE SURFACE)

The solution, based on the assumption that the coefficient of swelling equals the coefficient of consolidation, is given in Fig. 7. A comparison is made with the permeable-footing case and the one-dimensional solution and, although the rate of consolidation beneath the impermeable circular footing is definitely less than that of the permeable footing, it is interesting to note that the degree of consolidation beneath the impermeable footing is rather more than two-thirds that in the one-dimensional case even with a permeable footing, for all time-factors greater than about 0.1.

It is probable that the relation between \bar{U} and T given in Fig. 7 can be applied as a rough approximation to impermeable footings underlain by a clay stratum of thickness other than $1.6 \times 2R$.

CONSOLIDATION OF CLAY CORES IN EARTH DAMS

In engineering practice an important process of two-dimensional consolidation occurs in the case of hydraulic-fill dams. An analysis of this problem has been carried out by G. Gilboy¹¹ for a triangular core with a base angle of 45 degrees resting upon an impermeable stratum, which yields a solution :

$$\bar{U} = 1 - \frac{24}{\pi^4} \left[\sum_{m=0}^{\infty} \sum_{n=0}^{\infty} \frac{2}{(2n+1)^2 (2m+1)^2} e^{-[(2n+1)^2 + (2m+1)^2] \frac{\pi^2 T}{2}} + \sum_{n=0}^{\infty} \frac{1}{(2n+1)^4} e^{-(2n+1)^2 \pi^2 T} \right]$$

where $T = \frac{ct}{b^2}$ and $2b$ denotes the core base width. It may be shown that when the base angle of the core is 90 degrees the solution is that of the one-dimensional case (see equation (4a)).

As a means of predicting the rate of consolidation of cores with intermediate base angles, Gilboy proposed an interpolation formula :

$$T_{\alpha} = T_{90} \cot \alpha (T_{90} - T_{45})$$

where α denotes the base angle of the core.

In order to examine the validity of this formula and to extend the above solution, numerical analyses were applied to cores with base angles of 45, 60, and 75 degrees. The equation controlling the pore-water pressure is :

$$\frac{\partial u}{\partial t} = c \left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial z^2} \right) \quad . \quad . \quad . \quad . \quad (14)$$

If a square mesh in the x, z plane is used for the latter two cases the nodes of the mesh do not lie on the inclined faces of the core. This is a disadvantage from a computational point of view and in order to overcome this difficulty it is convenient to transform the above equation by the substitution :

$$z = w \tan \alpha$$

where w and z denote co-ordinates measured vertically from the centre of the base. Then :

$$\frac{\partial u}{\partial t} = c \left(\frac{\partial^2 u}{\partial x^2} + \cot^2 \alpha \frac{\partial^2 u}{\partial w^2} \right)$$

Thus, in the x, w plane all cores are transformed into triangles with a base angle of 45 degrees, and therefore the difficulty mentioned above does not arise.

The boundary conditions in the x, w plane are :—

(1) Along the inclined faces $u = 0$, $t > 0$

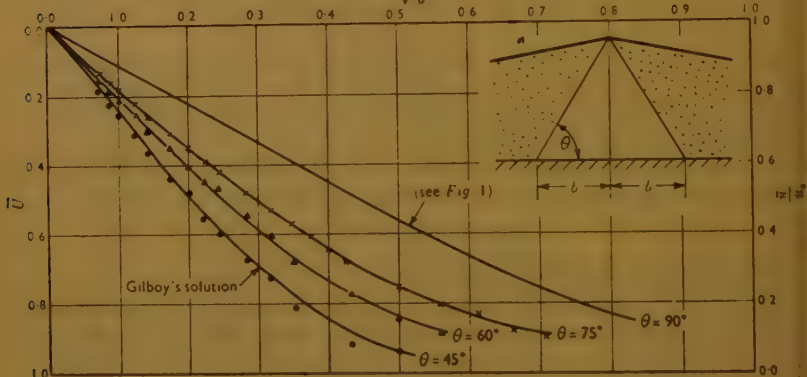
(2) Where $w = 0$, $\frac{\partial u}{\partial w} = 0$ $t \geq 0$

In order to effect a comparison with Gilboy's solution his assumption with regard to the initial values of the pore-water pressure was made, namely, that the pore-water pressure at a point is proportional to its vertical depth below the apex of the core. This question has been investigated in some detail by A. W. Bishop¹² and his conclusions indicate that the assumption is sufficiently good for all engineering purposes.

A solution has been obtained for the three cases mentioned above, and the average rates of consolidation determined (Fig. 8), the average

Fig. 8

$$\sqrt{T} = \sqrt{\frac{c_v l}{b^2}}$$



$\bar{U}-T$ CURVES FOR CONSOLIDATION OF A HYDRAULIC-FILL CORE IN A DAM

degree of consolidation in the core being connected with the average pore-water pressure \bar{u} by the equation :

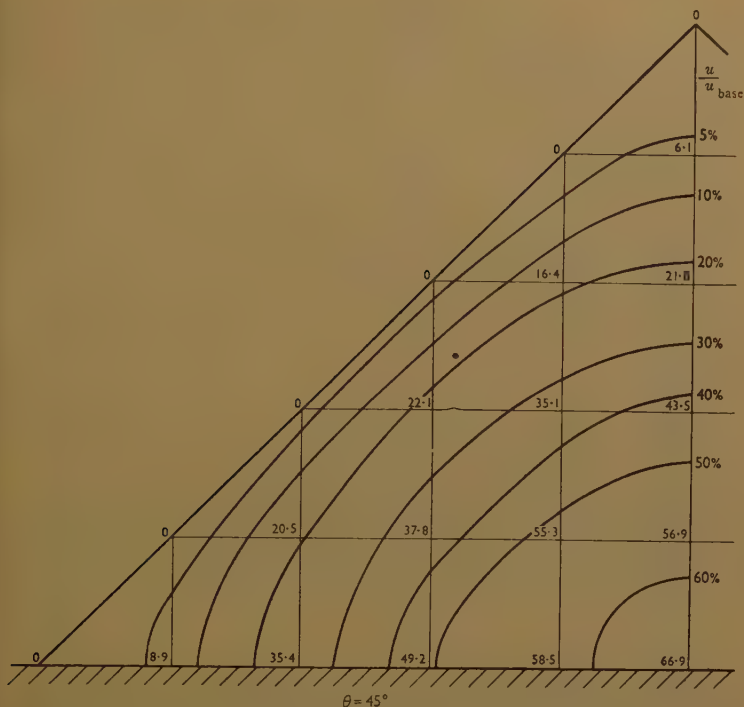
$$\bar{U} = 1 - \frac{\bar{u}}{\bar{u}_0}$$

In stability analyses it may be necessary to know the distribution of pore-water pressure in the core, and a typical distribution is shown in Fig. 9 for the case where $\alpha = 45$ degrees, at a time factor $T = 0.06$ (the initial pore-water pressure at the base of the core being equal to 100 units).

It will be seen that good agreement with Gilboy's solution has been obtained for the 45-degree core. The interpolation formula proposed by Gilboy was not, however, found to be valid.

It must be emphasized, however, that the case considered above is rather artificial, since in practice the greater proportion of the consolidation occurs during the construction period. In this case the consolidation equation (14) must be modified by the inclusion of a term involving the rate of construction, and account must be taken of the fact that the disposition of the drainage surfaces is changing with time. A numerical

Fig. 9



CONTOURS OF EQUI-PORE-PRESSURE FOR HYDRAULIC FILL OF A DAM AT A TIME FACTOR OF $T = 0.06$

solution could be obtained to this problem where an exact solution would be virtually impossible.

CONCLUSIONS

It has been shown that results in good agreement with existing exact solutions of consolidation problems may be found by using numerical methods. The advantage of such methods lies in their ability to take into account features in the problem which would have to be excluded or over-simplified in an analytical solution. The accuracy obtainable is quite sufficient for all engineering purposes.

ACKNOWLEDGEMENTS

The work described in this Paper was carried out in the Civil Engineering Department, Imperial College, London.

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The Paper is accompanied by six sheets of diagrams, from which the Figures in the text have been prepared, and by the following Appendices.

APPENDIX I

NOTATION

x, y, z	denotes	Cartesian co-ordinates.
$\delta x, \delta y, \delta z$	"	length of mesh sides.
z, r, θ	"	cylindrical co-ordinates.
$\delta z, \delta r, \delta \theta, h$	"	length of mesh sides.
t	"	time.
δt	"	time interval.
u	"	excess hydrostatic pressure in the pore-water.
c_v	"	coefficient of one-dimensional consolidation.
c	"	coefficient of two- or three-dimensional consolidation.
$\sigma_{xx}, \sigma_{yy}, \text{etc.}$	"	normal total stresses.
$\sigma'_{xx}, \sigma'_{yy}, \text{etc.}$	"	normal effective stresses.
$\sigma_1, \sigma_2, \sigma_3$	"	principal total stresses.
θ	"	sum of normal total stresses ($\sigma_{xx} + \sigma_{yy} + \sigma_{zz}$).
θ'	"	sum of normal effective stresses ($\sigma'_{xx} + \sigma'_{yy} + \sigma'_{zz}$).
$e_{xx}, e_{yy}, \text{etc.}$	"	normal strains.
Δ	"	sum of normal strains ($e_{xx} + e_{yy} + e_{zz}$).
k	"	permeability coefficient of the soil.
μ	"	Poisson's Ratio for the soil structure.
E	"	Young's Modulus for the soil structure.

\bar{U}	„	average degree of consolidation.
T	„	time factor.
$2H$	„	thickness of consolidation sample.
u_0	„	initial pore-water pressure.
β	=	$\frac{c \cdot \delta t}{h^2}$
m	=	$H/\delta z$
n	=	$t/\delta t$
p	=	$\frac{r}{\delta r}$
R	denotes	radius of cylinder.
λ	„	ratio of compressibility to expansibility of soil structure.
$2b$	„	base width of hydraulic-fill dam core.
α	„	base angle of core.
w	=	$z/\tan \alpha$.

APPENDIX II

GENERAL EQUATIONS OF CONSOLIDATION

In the general theory of consolidation as developed by Biot,² three differential equations are derived relating the displacements ρ_x , ρ_y , and ρ_z to the pore-water pressure u . The connexion between these equations and the familiar Terzaghi equation is not apparent. In the following the Biot equations are cast into a form that makes this connexion clear. The strains are assumed to be connected with the effective stress changes by equations such as :

$$e_{xx} = \frac{(1 + \mu)}{E} \sigma'_{xx} - \frac{\mu}{E} \theta' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where the effective and total stress changes are related by equations such as :

$$\sigma'_{xx} = \sigma_{xx} - u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Adding equations (15) :

$$(e_{xx} + e_{yy} + e_{zz}) = \frac{(1 - 2\mu)}{E} \theta'$$

Adding equations (16) :

$$\theta' = \theta - 3u$$

Thus :

$$\frac{E\Delta}{3(1 - 2\mu)} = \frac{1}{3} \theta - u$$

Further, by assuming d'Arcy's law to be valid, it may be shown that :

$$k \nabla^2 u + \frac{\partial \Delta}{\partial t} = 0$$

where

$$\nabla^2 \equiv \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}$$

Thus :

$$- \frac{1}{3} \frac{\partial \theta}{\partial t} + \frac{\partial u}{\partial t} = c \nabla^2 u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where

$$c = \frac{kE}{3(1 - 2\mu)}$$

Equation (17) is the general equation of consolidation. It may further be noted that, in the case of the oedometer test, $e_{xx} = e_{yy} = 0$ and it may be shown that :

$$-\frac{\partial \sigma_{zz}}{\partial t} + \frac{\partial u}{\partial t} = c_v \frac{\partial^2 u}{\partial z^2}$$

where $c_v = \frac{kE(1-\mu)}{(1+\mu)(1-2\mu)}$

When the consolidation pressure σ_{xx} is constant, as is generally the case, complete agreement is obtained with Terzaghi's one-dimensional equation.

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“ Problems in the Design and Construction of Knockendon Dam ” †

by

James Arthur Banks, O.B.E., M.I.C.E.

Correspondence

Dr W. L. Lowe-Brown felt that the Paper constituted an extremely valuable contribution to the Proceedings, as did all Papers which told of trouble met in carrying out work. It was a pity that it was not considered obligatory for an engineer to describe his troubles as well as his successes, because much more could be learnt from them. The present Paper might be looked upon as a worked-out example of the action to be taken when unexpected difficulties arose in carrying out work after the contract was well under way. Courage and good judgement were necessary to look the facts squarely in the face and admit that a change of design had become necessary, and then careful investigation was required to find a remedy. When the dam had been originally designed, soil mechanics methods had been unknown. Now they were sought out and, with the always willing assistance of the Department of Industrial Research to those in trouble and the trained co-operation of Soil Mechanics Limited, a satisfactory solution based upon sound engineering judgement and common sense was found. Dr Lowe-Brown could say from experience how helpful both those bodies were.

There were two points, both concerning puddle, to which he wished to refer. The first was in connexion with the method of analysis by the use of circular arcs for slip surfaces. In the Swedish method for calculating the stability of earth slopes, Dr Fellenius used a circular arc as an approximation to the actual slip surface; in homogeneous material a single radius was used, but when several kinds of ground were passed through, a different radius was taken for each. Nowadays the practice had grown up of using one radius for a curve passing through several different materials such as puddle, semi-impervious fill, and pervious material.

† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 423 (July 1952)

That was admittedly an approximation in order to simplify the calculation, but it should only be used as a first approximation because, in Dr Lowe-Brown's opinion, it was an over-simplification. An unmatured puddle wall had such very different characteristics from a consolidated bank that, he suggested, if the calculation were made in two parts it would be easier to see exactly what was being done. The horizontal pressure exerted by the disruptive action of the puddle wall at the first vertical slice of the consolidated bank downstream could first be calculated, and then the resistance to sliding of that bank assessed to see if it was equal to the task. The Author had asked if the pressures given by Bell's formula were really developed, and the same question had been asked during the discussion on the Eildon Dam. Bell's formula for the horizontal pressure was based on the assumption that failure took place on a plane of rupture inclined at some angle to the horizontal, so it could hardly be expected to give the same result as was found between vertical slices in a failure along a circular slip curve. Messrs Cooling and Golder¹ had given a method of calculating the pressure between any two vertical slices. If the calculation was made for the pressure between the last slice in the puddle and the first in the consolidated material, a pressure not far removed from that given by Bell's formula would be found. Dr Lowe-Brown was rather surprised that in only one of the slip curves given in the Paper was a separate radius taken through the puddle—that was, curve q—and in that case he had made a rough check to see what the pressure was, and to do that it had been necessary to make certain assumptions. The result showed much closer agreement with that obtained by Bell's formula than was to be expected for a method based upon such different hypotheses. If the Author would give the result from the more accurate figures at his disposal it would be most interesting. Dr Lowe-Brown did not believe it was generally recognized that when using the Swedish method such high pressures between the slices were implicit.

The part played by the disruptive effect of the puddle wall in the slips at both Chigwell Dam in Essex and the Eildon Dam in Australia was still an open question with some engineers, and that question was further complicated by the pore pressure.

The curves of pore pressures given in the Paper were most interesting, and in the investigation for the enlarged Eildon Dam in Australia² elaborate apparatus had been used for that purpose.

The second point that he wished to raise concerned the consistency of the puddle used. On p. 437, the Author had drawn attention to the very marked drop in the strength of boulder clay with a small percentage increase in the water content, and stated that, for clay that could be properly

¹ "Analysis of the Failure of an Earth Dam during Construction." J. Instn Civ. Engrs, vol. 19, p. 38 (Nov. 1942).

² M. G. Speedie, "Investigations and Designs for Eildon Dam Enlargement." J. Instn Engrs Austr., vol. 20, p. 81 (July-Aug. 1948).

puddled, the shear strength was reduced to 2 lb. per square inch (which would indicate a water content of about 25 per cent), and that such puddle restricted the use of consolidating equipment in the zones of the embankment adjacent to the core. On p. 436 the Author had said that particular care had been taken during the construction to keep the water content down to the minimum and as the work had progressed the method of forming the watertight core had been altered. Apparently, that modification had been made at mid-height of the dam. At that point souring was dispensed with (it was not mentioned whether the puddled clay was passed through a pug mill or not) and boulder clay was spread across the whole width of the embankment. Along the line of the puddle core it was watered "cautiously" so that the consolidating plant could be used in the central zone as well as on the rest of the embankment. That seemed to indicate that the ordinary puddling practice of placing puddle by heeling had been abandoned, thus breaking with convention, and that the clay had been placed in bulk and rolled. Would the Author give a more detailed description of what had been done and say what had been the resulting water content of the clay in that central zone?

The question of how wet or how dry puddle should be to make a satisfactory job was one on which different views were held by experienced engineers, and the Author's experience in that particular case would be most interesting.

Dr H. Q. Golder, referring to the lateral pressure of the puddle clay mentioned by the Author on p. 436, observed that the conditions at Knockendon were different from those at Muirhead. At Knockendon it was quite possible that some such silo effect, as described by the Author, had in fact occurred. However, that effect could only be present if the sides of the silo were comparatively rigid. At Muirhead that was far from being the case. The lowest layer of material had softened to such an extent that the firmer material above it could yield in a horizontal direction under the action of the puddle pressure. In that case, therefore, it had been necessary to re-design the bank so that the factor of safety against puddle pressure was at least greater than one.

A silo effect could not reduce the weight of the puddle, which was the cause of the lateral pressure. All that a silo effect could do was to reduce the pressure on the base of the silo and the bursting effect near the bottom. That was only done at the expense of increasing the lateral pressure higher up. The full lateral pressure had in some way to be resisted by a horizontal force in the dam structure above ground level, since the dam was, in that respect, virtually a two-dimensional structure, unlike a silo.

The opinion of many experienced engineers had changed in the past 10 years on the question of whether or not the layers in which the material of an earth bank was placed should slope inwards to the centre or outwards to the side slopes. Presumably the reason which had originally accounted

for the inward slope was that failure might take place by sliding along the joints between the outward-sloping layers. Thus the inward slope would prove more stable.

With modern methods of placing earth, particularly a boulder clay of the type used at Knockendon and Muirhead, it was impossible to see any construction joints in the material if a trial pit was dug after compaction. The outward slope, therefore, was just as safe as the inward slope. The outward slope had the further great advantage that water was shed from the bank. With a low-liquid-limit clay that was of the greatest practical importance.

At the time that work had been carried out, little had been known about the effect of pore-water pressures. The famous case of the San Francisco-Oakland Bay Bridge, in which the pore-water pressure was demonstrated by the water level in a measuring tube standing above groundwater level, was of course well known. However, so far as Dr Golder was aware, the observations made at Knockendon were the first in Great Britain to reveal the water level standing above ground level.

The first indication of that phenomenon had been a report from an intelligent, well trained, but incredulous foreman that he had left the boring tubes projecting 2 feet above ground level and, during the night, water had risen in them and overflowed. Although rain had fallen in the night that could not account for the quantity of water which had filled the tubes. The Author and Dr Golder had immediately proceeded to the site to investigate the report. It had been observed that the foreman had, in fact, reported correctly. Measuring tubes of 3 inches diameter had therefore been inserted in the borehole and the observations reported had been commenced.

The Author's statement that "in tube No. 2 the fact that the water level rose above the finished level of the embankment at the tube may have been partly due to the greater height of the embankment beyond the tube," might of course be true, but was not necessary. Theoretically, the water level could rise to almost twice the finished level, since the total weight of the saturated compacted material would be almost twice that of water. Thus, if the whole of the total pressure were carried by the water, the effective pressure would be zero, and the height of water in the tube would be twice the height of the bank.

Dr Golder could not agree with the Author in his suggestion that the instability at Muirhead had been caused by an increase in pore-water pressure, such as had been observed at Knockendon. In Dr Golder's opinion, the trouble at Muirhead had been that the lowest layers of fill had been placed without adequate compaction. That of course had been done many years ago, before the necessity for careful compaction had been realized, and it was possible that if construction had proceeded at the original rate the bank might have been successfully completed. However, it was certain that the upper material placed at Muirhead by large modern

earth-moving equipment was much better compacted than the lower material.

Trial pits put down through the upper material into the lower material had shown that the latter was so soft that lorries could not run over it. Since it had been placed by lorries running over it that was conclusive proof that the material had softened since placing. That, of course, implied a large air-void content in the material, thus allowing softening to take place when water penetrated to it. It was not necessary in that case to postulate excess pore-water pressure. In fact, if the material contained large air voids, excess pore-water pressures would be impossible since those could only occur in a saturated or nearly saturated material.

The problem in design, of course, was to predict the strength which the material would have in the bank after compaction. A laboratory approach could never give the final answer to that problem since so much depended on the construction equipment, degree of control during placing, and the weather conditions. However, much could be done by laboratory tests, and the worst conditions could be simulated. An attempt could be made to estimate the pore-water pressures resulting from the weight of the bank material placed. The rate at which they would dissipate could also be estimated. However, the unknown factor was not pore-water pressures resulting from the weight of the bank, but whether or not it was possible to increase those pressures by the use of extra heavy compaction equipment. Theoretically, that was so, although there existed an upper limit which was presumably a function of the shear strength of the material. Thus, there might be reasons why, in certain soils, extremely heavy equipment should not be used.

Dr L. F. Cooling thought that the measurements of pore-water pressure carried out in the earth dam at Knockendon had probably been the first of their kind in Great Britain. As the Author had indicated, the apparatus and techniques used had been somewhat crude and elementary, but even so the results had been sufficient to indicate the potential value of such observations. Dr Cooling would like to support the Author's plea that more serious attention should be given to that aspect of earth-dam construction, but at the same time he would like to point out that investigations on that problem were already under way in Great Britain. As part of its field research programme, the Building Research Station was carrying out pore-water pressure measurements on three earth dams at present under construction, employing techniques which had already been found satisfactory on other problems of a similar nature.

The pore-water installation was similar in principle to that used by the United States Bureau of Reclamation.¹ At selected levels during the placing of the embankment, shallow trenches were dug for the purpose of installing the piezometers and their connecting tubing. Each piezometer

¹ F. C. Walker and W. W. Daehn, "Ten Years of Pore Pressure Movements." Proc. 2nd Int. Conf. Soil Mech. and Foundn Engng, 1948, vol. III, p. 245.

consisted of a porous stone fitted into a holder made of plastic ; that was placed face downwards on the bank material and from it ran two water-filled plastic tubes connecting the porous stone to an instrument panel housed at some convenient place outside the dam. At each level one main trench, carefully backfilled, served to carry the tubes from several peizometers through the body of the dam. The instrument panel consisted of de-airing equipment and a set of Bourdon gauges to measure the pressure developed. The apparatus was fairly straightforward and the main difficulty in obtaining correct pore-pressure readings was to ensure that the system was completely air free. The provision of two tubes to each pressure point was found to be essential for that purpose. A complete installation could be fitted up for a few hundred pounds, and for any dam in which the material was inclined to be impervious, the insight which pore-pressure observations gave as to the behaviour of the soil in the body of the dam was of important practical value.

Such observations served not only as a control and a warning of possible instability during construction but, in Dr Cooling's opinion, gave the most promising means of building up a fund of data on which improvements in design procedure could be based. It would be impossible to rely entirely on data gained from experience in other countries where the conditions, climatic and otherwise, might be very different.

The factors which the engineer had to consider in designing a dam, such as type of construction equipment available, weather conditions, the probable degree of control during placing, rate of building, and length of the construction season were all factors likely to influence the pore pressure developed during the construction of an embankment with a material of relatively low permeability. Hence, in assessing his design strength, the engineer had to make some assumption, tacit or otherwise, as to the probable trend of pore-pressure development during construction.

At Knockendon the Author had based his design on a strength equal to that in the existing lower fill and had not counted on any increase in strength during construction, thereby assuming a development of 100-per-cent pore pressure for additional loads on the lower fill. His measurements had given a more optimistic picture, and with it the reassurance that the actual factor of safety was higher than that which he had adopted for design. Other cases could well be imagined where readings of high pore-water pressure might give timely warning of the possibility of dangerous conditions developing, and so permit the taking of appropriate corrective measures before failure occurred.

The Author had also mentioned the question of whether the puddle core could be dispensed with in a dam like Knockendon, which was formed of relatively impervious material. The slow dissipation of pore pressure in the body of the dam, as indicated in *Fig. 13*, certainly gave point to the question. Dr Cooling was aware that there were arguments on both sides and that other factors had to be considered. He would like to suggest that,

if pore-pressure measurements could be taken along a section passing both upstream and downstream of the puddle core and continued for a period after filling a reservoir, it would give factual information as to how far the puddle was functioning as a water-stop in the particular case and would help appreciably in resolving the problem.

The Author, in reply, observed that Dr Lowe-Brown, referring to the method of analysis by the use of circular arcs for slip surfaces, had expressed the opinion that a single-circle analysis was an over-simplification where the curve passed through several different materials. Many single- and compound-circle curves had been analysed, the method used being that given by Messrs Cooling and Golder, to which Dr Lowe-Brown had referred. The curves shown in *Figs 9* and *10* had been found by trial to be the criteria for design of the dam, and the curve *q* (*Fig. 10 (b)*) had proved to be the only compound curve that came into that category. The calculation on a double-circle curve was, in fact, made in two parts to overcome the difficulty arising from a change in radius, but in the Author's conception the same principle was inherent in the single-circle analysis with dependable results, provided that appropriate shear strengths were taken for the different zones of materials through which the circle passed. The only simplification was that the weight of puddle clay had been taken as being the same as the banking material, and that had been conservative.

In response to Dr Lowe-Brown's remarks concerning the curve *q*, the horizontal force on the right-hand plane forming the junction between the puddle core and the embankment proper (*Fig. 10 (b)*) had been calculated by the slip-circle method to be approximately 200,000 lb. per foot run of embankment. Using Bell's formula, the horizontal force on that plane had been found to be 270,000 lb. If the whole of the material traversed by circle *q* to the left of the plane had the same cohesive value as the puddle core, namely, 2 lb. per square inch, then the result obtained by both methods would have been practically the same. Since, however, the embankment material had the much higher cohesive value of 7.6 lb. per square inch, the analysis seemed in the Author's view to cast doubts on the application of the Bell formula in assessing the horizontal force in an earth embankment resulting from a comparatively thin puddle wall.

Regarding the consistency of the puddle clay, the normal procedure followed had been to excavate the clay and leave it exposed to the weather until it had absorbed sufficient moisture in order that, when placed in the trench and worked with deep spades, it formed a thoroughly homogeneous core. The clay had not been passed through a pug mill. In the Author's experience, it had not been the practice to mill the hill clays generally available for reservoir work in Scotland, and he considered that the presence of a percentage of stone in the clay was not a disadvantage.

The modified procedure ultimately adopted in forming the puddle core had been to excavate the boulder clay from the borrow-pit and spread it

in layers across the embankment without any break at the puddle core. Bulldozers, operating continuously, had been used to compact the material and, in the central zone representing the width of the normal puddle core, a bulldozer with new tracks had travelled longitudinally across the embankment, that zone being watered with a rose but only sufficiently to ensure from observation that the material was worked into a plastic condition by the penetration of the bulldozer track. That procedure was a break with convention, as Dr Lowe-Brown had suggested, and was in the Author's opinion more than justified where a retentive material had to be used for forming the embankment as a whole. If a less retentive material had been available to form the Knockendon embankment, it was probable that the more orthodox procedure adopted at the outset in forming the lower part of the core would have been continued. It should, however, be noted, as Dr Golder had observed, that it was impossible to see construction joints in the banking material when a trial pit was dug after compaction, and it might be that a thoroughly homogeneous puddle core could be obtained in any case by means less laborious than the cutting with deep spades which had been the orthodox procedure in the past. Whatever laboratory tests might indicate, the experienced practical judgement of the engineer was an important factor in determining what constituted a satisfactory job.

The resulting water content of the clay in the central zone of the Knockendon embankment was in the range 21 to 22 per cent, indicating a shear strength of $3\frac{1}{2}$ to 4 lb. per square inch. However, the gain in strength of the puddle core was a less important advantage than the assurance of proper compaction in the zones of the embankment immediately adjacent. The difficulty of obtaining adequate compaction against a soft puddle core might have serious results on the overall stability of the embankment and, in the Author's opinion, that was at least a contributory factor to the deterioration which had developed in the Muirhead embankment. With reference to that, Dr Golder had observed, perhaps tactfully, that the necessity for careful compaction had not been realized until more recent years. The Author thought it was true to say that the need for firm compaction had always been recognized, although the possible after-effects of inadequate compaction had not been fully appreciated until the advent of soil mechanics. The more gradual mode of construction in the past had provided a safeguard to compensate for the lack of technical knowledge, although it was evident from the distortion apparent in the slopes of many old embankments that the factor of safety at the time of construction had been precariously low.

On the question of lateral pressure of the puddle clay, the Author agreed that the conditions at Muirhead had been less favourable in that respect than at Knockendon, and concurred in Dr Golder's deduction that in modifying the design of the Muirhead embankment it had been necessary to provide against horizontal shear from puddle pressure.

With regard to the possible silo, or rather "bin," effect pertaining to a puddle core, the Author in his investigation had not reached any positive conclusions, but had been sufficiently confident that full lateral pressure would not be realized at Knockendon to discount it to a great extent in determining the ultimate profile of the embankment. It was significant to observe that, if full lateral pressure in a horizontal plane as determined from Bell's formula was developed, then that, rather than a shear circle analysis, would in most cases be the criterion for embankment design.

With regard to the inclination of the layers in which the material of an earth bank was placed, the practice in the past of making an inclination inwards towards the centre of the embankment had most probably been prompted to give greater stability against sliding along the joints, as Dr Golder had suggested. A further reason was that the "fines" tended to drain towards the heart of the embankment to give greater density there, and also it was believed that by draining towards the centre the puddle-clay core was maintained to advantage in a moist condition. The customary mode of construction of the embankment might have been prescribed to ensure that the layers would incline inwards but, on the other hand, it might be that the inclination was determined by the construction procedure. It was usual to specify that the embankment should be formed by laying light-gauge track commencing at the outer edge of the slopes, and using tipping wagons of 1 cubic yard capacity. The track was moved inwards when sufficient material had been deposited to form a new alignment and thus the mode of operation obstructed surface drainage outwards. That method, with the successive movement of track and the limited quantities of material dumped at any one time for spreading, gave a slow but sure process of compaction. It was somewhat easier to form an embankment to profile from the outside inwards than in the reverse direction, and the Author saw no reason why the accepted procedure in the past, adapted to suit larger transport vehicles and mechanical means of compaction, should not be continued, provided that the embankment was being formed of a non-retentive material which would ensure natural drainage. If a retentive material, such as boulder clay, had to be used, then it was imperative that the layers should slope outwards and adequate means be provided to prevent lodgement of surface water.

It was gratifying to learn from Dr Cooling of the activities of the Building Research Station concerning measurements of pore-water pressures and to have his description of the apparatus being used for that purpose. The value of having a fund of data derived from observations under the climatic conditions and pertaining to materials commonly available in Great Britain would be appreciated by all engineers concerned with the design of earth dams. No doubt, Dr Cooling or others concerned would in due course make known the information obtained. Dr Cooling's suggestion—that pore-water measurements should be taken upstream and downstream of the puddle core and continued for a period

after filling a reservoir should be followed, since information was meantime lacking as to whether the puddle core in an embankment was in itself the complete water stop or whether any substantial contribution was made by the embankment material closely adjacent to it.

The Author would like to thank Dr Lowe-Brown, Dr Golder, and Dr Cooling for the constructive observations they had made on his Paper. Their opinion, supported by wide experience in problems pertaining to earth structures, had been most helpful in elucidating some of the uncertainties, which were more numerous in that than in other spheres of civil engineering more mature in technical development.

Paper No. 5867

“The Skylon” †

by

**Felix James Samuely, B.Sc.(Eng.), A.M.I.C.E., and
Percy James Alfred Ward, A.M.I.E.E.**

Correspondence

Mr H. C. V. Woolard, of Wellington, New Zealand, observed that although it was attractively simple in conception, the type of structure described in the Paper, as the Authors had shown, confronted the designer with problems that required much enterprise and ingenuity for their solution.

Mr Woolard was much interested by the Authors' remarks on dynamic forces and the means which had been employed to minimize them. In the design of a number of lattice steel wireless masts with which he had been concerned, forces of that nature had not been thought to be of much importance and that view had appeared to be confirmed by the behaviour of the structures in practice. A major factor in that connexion, of course, was the ratio of the total wind load to the total structure weight and, in the case of an open lattice mast, that ratio would tend to be low. Table 2 showed some typical figures.

It would be interesting if the Authors would give the corresponding figures for the Skylon, particularly in view of their observation that the extra weight involved in the change-over from a light-alloy to a steel structure had been helpful in securing dynamic stability. Could the Authors say what “shock factor” they would have used if the light-alloy covering had been omitted?

† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 444 (July 1952).

TABLE 2

Height of mast	Total windage : tons	Total weight : tons	Windage/ Weight ratio
500 feet	48	88	0.55
750 feet	82	142	0.58

Among the unusual features of the Skylon, from an engineering point of view, was the fact that it was stayed at one point only (disregarding the bottom pennants), the stayed point being not somewhere near the top but at the centre. Wireless masts had, of course, been designed and occasionally built in that way, but from the standpoint of dynamic stability such an arrangement seemed far from ideal. A conventional mast, of course, would be stayed at a number of elevations and the stays, as compared with those of the Skylon, would be relatively slack. The several sets of stays, each group being of a different natural frequency, would no doubt have a useful damping effect on the structure as a whole. It would be interesting to have the Authors' comments on that aspect of the problem; perhaps they would say how they would have liked to arrange the stays on the Skylon had circumstances allowed them a free hand.

Mr Samuely, in reply, observed that the Skylon was, in the first instance, an architectural expression of what could be done nowadays in the way of structures, and whilst it was important to investigate whether a structure like that could be designed and erected, he had never considered the question of whether any other structure, which might possibly not have had the same architectural expression, would be better.

The term "dynamic forces" used in the Paper did not refer to the wind forces. If a big object like the Skylon was subject to heavy oscillations it would be consistently accelerated and decelerated; those accelerations and decelerations would produce forces and stresses in the supports which, in the case of the Skylon, exceeded very much the normal forces of that kind, because not only was the top expected to move very much more than usual, but also the bottom was subject to considerable displacement. Those excessive movements had been reduced to what was considered normal magnitudes by the use of prestressing, and in that way the dynamic forces were correspondingly decreased, as described in the Paper, to such an extent that they became unimportant.

Mr Samuely could not relate the ratio of the wind load to the total structure weight, because the dynamic forces increased with the weight of the structure.

According to Dr Piercey, the maximum shock factor that had ever needed to be taken into account was 1.8.

Mr Ward, in reply, observed that the claim made in the Paper,

namely, that the weight of the central feature acted as a stabilizing agent, referred only to its effect as a steady downward force on supporting pennants. Reference to the Appendices would show that a considerable force on the pennants was necessary to provide stability to the three leg members when side stays were omitted. As Mr Samuely had pointed out, the counter effect of the weight of the central feature upon dynamic stability was taken care of by prestressing, an important function of which was to reduce the variation in downward thrust upon supporting cables under the various conditions of loading.

OBITUARY

SIR LEOPOLD HALLIDAY SAVILE, K.C.B., who died on the 28th January, 1953, at the age of 82, was born on the 31st August, 1870.

He was educated at Marlborough College, and later (1888 to 1890) studied Applied Science at King's College, London. On leaving College he became articled as a pupil to Sir John Wolfe Barry and H. M. Brunel. On the completion of his pupilage in 1894, he remained with Sir John Wolfe Barry and Partners as Assistant Engineer, engaged on the new entrance works at Tyne Docks and at the new docks at Barry.

In 1896 he left England to spend 4 years in India—first as Assistant Engineer on the construction of the South Punjab railway, and from 1898–99 as Assistant Engineer on the construction of the Bengal and North Western Railway.

Returning to England in 1899 he became Resident Engineer on the lowering of the Ramsden Dock Sill at Barrow-in-Furness.

In 1902, he again went overseas, spending a year in Australia assisting in the preparation of the designs for a dock in New South Wales, and then went on to Singapore for a similar period, during which he assisted in the planning of the new docks there.

Although he returned to England in 1903 he remained for only a year, for in 1904 he was appointed Deputy Chief Engineer to the Bombay Port Trust and remained in India until 1919. During this time he was in charge of construction of the Alexandra Dock and Hughes Dry Dock.

During the first World War he served with the Gorakpur Light Horse and later with the Bombay Light Horse.

From 1919 he was Civil Engineer-in-Chief, Admiralty, until his retirement in 1932. Among the many major works under his control was the construction of the Singapore Naval Base. During 1932 he became a Partner in the firm of Sir Alexander Gibb and Partners, Consulting Engineers, Westminster, until his retiral in 1947 when he acted as Consultant to the firm until his death. Among the many major works that he dealt with when with the firm was the visit to Australia in 1939 to locate the best site for a major dry dock to be built by the Commonwealth Government for battleships and commercial vessels of the "Queen Mary" class. He selected a site in Sydney Harbour for this, the Captain Cook Graving Dock, built during 1940–1945 under his direction.

He served as a Member of the International Consultative Committee on the Suez Canal and of the Permanent International Association of Navigation Congresses.

He was made Companion of the Bath in 1925 and was created Knight Commander of the same Order in 1929.

He was the Author of several technical Papers on docks and harbour

engineering, including "Lowering the Sill of the Ramsden Dock, Barrow-in-Furness,"¹ for which he was awarded a Crompton Prize in 1904. and "Demolition of the Harbour and Defence Works of Heligoland,"² for which he received a George Stephenson Gold Medal in 1924.

Sir Leopold was elected an Associate Member of the Institution in 1895, and was transferred to the class of Member in 1914. He served on the Council from 1932 to 1945 and was President of the Institution for the Session 1940-41. He was also an Associate of the Institution of Naval Architects, and a Member of the Institution of Engineers, Australia.

He leaves a widow, and a daughter of a previous marriage.

PERCY WALTER BERTLIN, who died on the 16th October, 1952, at the age of 81, was born in London on the 14th June, 1871.

He was educated at the University College School, London. In 1889 he went to South America to become Assistant Engineer on the construction of the extension of the Central Argentine Railway.

Returning to England in 1891 he spent a year as pupil with Mr William Clarke of Westminster. On the completion of his pupilage he became Assistant Engineer to Mr Clarke during the construction of the Kingsbridge branch of the Great Western Railway.

In 1896 he joined the staff of Messrs John Aird and Sons as Assistant Engineer, and was employed on many different construction projects, including the first Staines Reservoir and the Woking Waterworks, Chertsey, where he was Managing Engineer.

In 1903 he left this firm to join Edmund Nuttall, Sons and Co. Ltd as Agent in Charge, and for the following 14 years was engaged on all types of construction works. In 1917 he left to become Civil Engineer to Viscount Furness, during the construction of Haverton Hill Shipyard; in 1918, however, he returned to Edmund Nuttall, Sons and Co. Ltd as Managing Engineer on the construction of the Brough seaplane station. Then followed a year in charge of the construction of the New Tranmere Dock, Birkenhead, upon which subject Mr Bertlin read a Paper before the Institution—"Construction of the New Entrance to Tranmere Dock."³

In 1923 he was appointed to a Directorship of the Company, becoming Managing Director in 1938, which position he held until his retirement in 1944. During the years of his Directorship the firm carried out a number of large civil engineering contracts, and at one period he was Resident Director in charge of their important contracts for the construction of the road tunnel under the River Mersey, and the approaches to same on the Liverpool side; he was also closely associated with the construction of the King George V Graving Dock at Southampton.

¹ Min. Proc. Instn Civ. Engrs, vol. 158 (1903-04, Pt. IV), p. 106.

² Min. Proc. Instn Civ. Engrs, vol. 220 (1924-25, Pt. II), p. 55.

³ Min. Proc. Instn Civ. Engrs, vol. 221 (1925-1926, Pt I), p. 241.

Mr Bertlin was elected an Associate Member of the Institution in 1897, and was transferred to the class of Member in 1926. He was Chairman of the Manchester and District Association of the Institution for the year 1932-1933.

He leaves two sons and a daughter.

JOHN WOOD, C.M.G., who died at his home at Paraparaumu, New Zealand, on the 12th July, 1952, at the age of 71, was born at Adelaide on the 15th December, 1880.

Educated at Timaru High School, South Island, New Zealand, he spent 4 years—from 1900 to 1904—obtaining practical experience in the Public Works Department of New Zealand.

The initial part of his training over, almost the whole of the following 16 years (1904-1920) were spent as Assistant Engineer on various works of railway construction throughout North Island, the last 8 years in particular being spent as District Engineer in charge of Whangarei District, during the execution of a large road and railway construction project.

In 1920 he joined the Public Works Department at Wellington as Inspecting Engineer, being promoted to Assistant Engineer-in-Chief in 1932.

In 1936 Mr Wood was appointed Engineer-in-Chief and Under Secretary, of the Public Works Department, an appointment which he held until his retirement in 1941.

In recognition of his services he was appointed, in 1938, a Companion of the Order of St Michael and St George.

Mr Wood was elected an Associate Member of the Institution in 1907, and was transferred to the class of Member in 1936. He was a Member of Council, resident in New Zealand, from 1942-1945.

He leaves a widow, four daughters, and one son.

CORRIGENDUM

Proceedings, Part I, January 1953.

p. 42, line 20, delete "CHARLES ALEXANDER CHAMBERLIN
(E. 1912)."

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